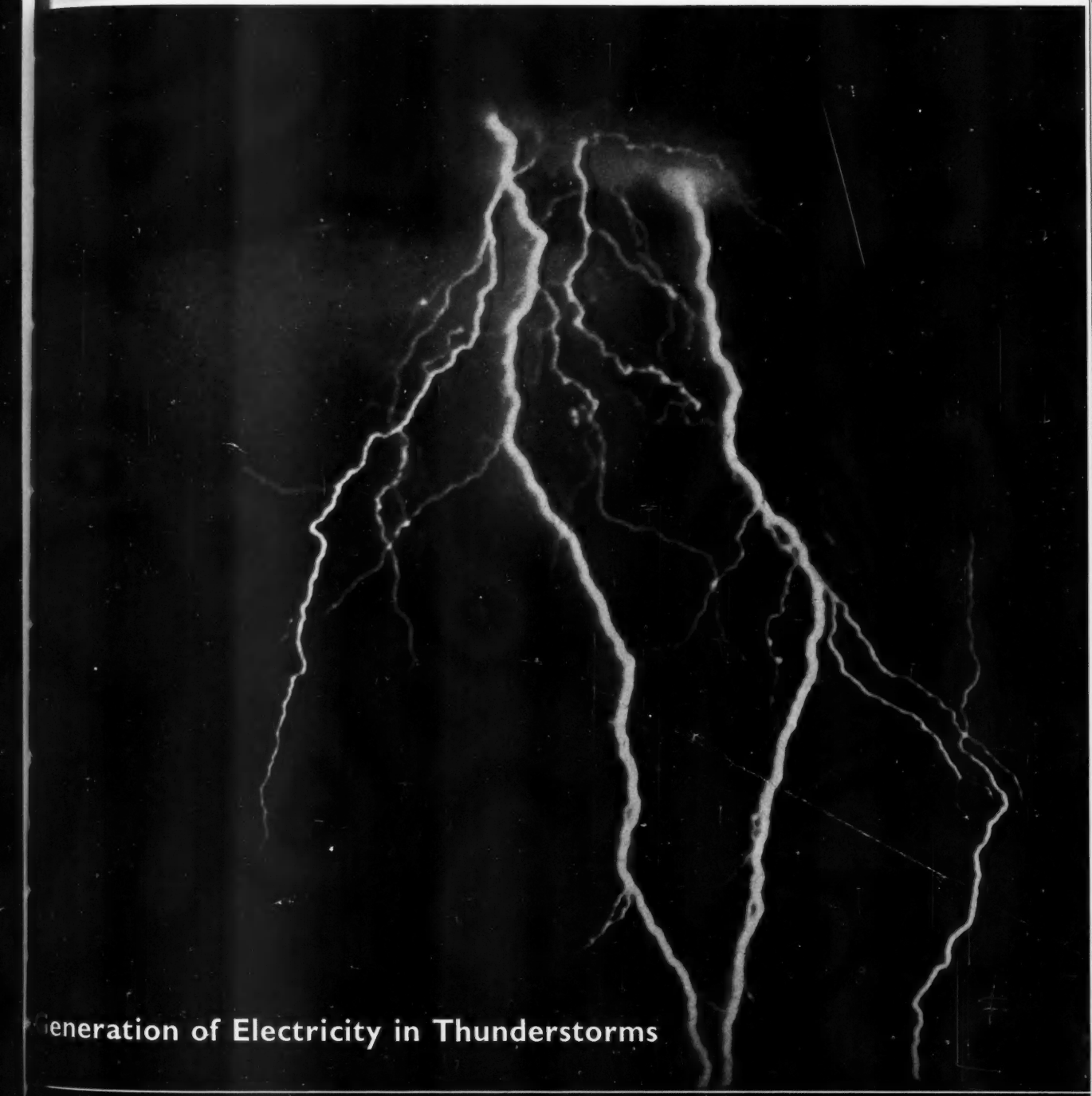
DISCOVERY

THE MAGAZINE OF SCIENTIFIC PROGRESS

APRIL 1961 3/-



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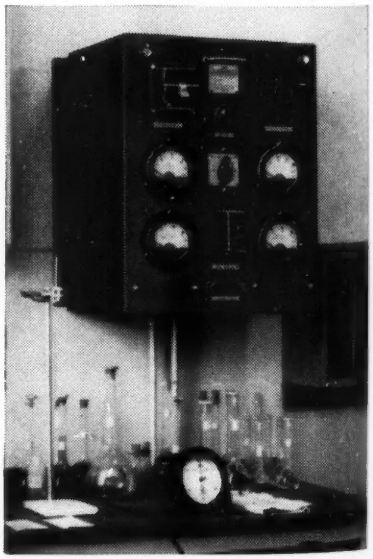

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the glyceride structure of fats

Jack Sprat, they say, could eat no fat. One can only speculate on the metabolic disorders which led to this curious condition and wonder if our present knowledge could have helped produce something more suited to his digestive system. For we know a great deal now, and much of it is set out in the collective writings of Bailey, Deuel, Hilditch and Lovern. Yet the things we do not know will provide fruitful ground for research for many years to come.

The structure of glycerides alone—basically simple but infinitely variable—is a matter of perennial interest whether one is concerned with natural occurrence, chemical formation, physical behaviour or biological purpose. How significant is the use of a trihydric alcohol? Why are natural fats so specifically constituted? Is it fortuitous or predestined that 1-oleo-distearin and 2-oleo-distearin should have such contrasted physical properties? Do species of natural occurrence represent the limit of useful glycerides? How much can a study of model systems contribute to an understanding of natural products? To what extent does structure influence emulsion systems?

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OUR COVER PICTURE

Lightning strokes travelling along branched paths from cloud-base to ground. Some branches apparently terminate in the air without reaching the ground.
(*Photographed in Scotland by J. F. Steljes.*)

THE PROGRESS OF SCIENCE

MARS COVERED WITH ICE?

The possibility that "canals" on Mars may be great fissures in a thick layer of ice has been put forward by Soviet astronomer V. Davydov, scientific secretary of the Sternberg Astronomical Institute.

Davydov believes the quantity of water per unit area on Mars could be comparable with that on the Earth, or even greater. With temperature conditions as they are on that planet, its oceans should be covered with a layer of ice that is 300 m. thick at the equator and increases to a thickness of 2000 m. at the poles, he points out.

Why doesn't Mars appear to be a solid ball of ice to the observer? He says it has most likely become covered with sand and dust during the past hundreds of millions of years. Earthquakes (or rather, Mars-quakes) have produced fissures in the ice that have exposed the ground below and it is in these exposed areas that Martian vegetation has been primarily concentrated.

MALARIA ERADICATION PROGRAMMES AROUND THE WORLD

Of the 1336 million people that have been living in areas infected by malaria, 298 million (22·3%) are now living in regions from which this disease has been eliminated and another 170 million (12·7%) are in areas in which preliminary eradication activities are in progress. Some 612 million more (45·9%) are in regions that are included in planned eradication programmes, leaving 255 million (19·1%) in areas still to be dealt with.

These figures were contained in a report presented at the recent meeting of the WHO governing body in New Delhi. The report went on to state that the over-all programme was lagging behind schedule for a number of reasons—that "while it is relatively easy to find solutions to technical and operational shortcomings, it is much more difficult to correct administrative imperfections".

The advances made in Europe indicate the consolidation phase will be reached everywhere by the end of 1962.

In South-east Asia, progress is not uniform; in India, which has the largest malaria eradication programme in the world, there has been a considerable decline in incidence and the over-all picture appears satisfactory, but there are sections in which infection rates are still high.

In Africa, well-planned pilot projects show it is technically feasible to bring the disease under control despite the "very intense" transmission.

In the Americas there has been considerable progress in most programmes and the prospects of eradication are hopeful. There has also been progress in the majority of eastern Mediterranean countries.

In the western Pacific, all of the countries have agreed in principle to malaria eradication. Plans are fully worked out in only two of them, however, although malaria has disappeared or become negligible in some others. Medicated salt is being tried in Netherlands New Guinea and Cambodia, and an attempt to control mosquito larvae by the introduction of a parasitic fungus is being made in the Tokelau islands.

MACH 50 SHOCK TUNNEL

A shock tunnel that is expected to be capable of testing two models simultaneously at speeds up to fifty times the speed of sound has been developed by the General Electric Co. (U.S.A.); it has already been taken up to thirty-five times the speed of sound.

These extreme velocities are produced by exploding a mixture of hydrogen and oxygen diluted with helium in a closed section at one end of the tube. The explosion ruptures a stainless steel diaphragm separating the burning mixture from the air in the 100-ft. length of tube and sends a shock wave travelling down to the other end. The air, compressed to a high pressure and temperature, is then forced out through two separate nozzles over the two small test models.

Under standard atmospheric conditions (60°F), the speed of sound is about 760 m.p.h.; Mach 35 would be about 26,500 m.p.h. under these conditions and Mach 50 would be 38,000 m.p.h. (At higher temperatures, the speed of sound is higher.) These velocities are well above the 25,000 m.p.h. velocity needed by a rocket to escape the Earth's gravitational field (after power cut-off and for short burn-times, as is usually the case at present).

General Electric engineers state that practically any desired atmospheric condition can be provided for tests. They are now simulating space-vehicle entry into artificial atmospheres resembling those of Venus and Mars.

THE U.S.A.'s METEOROLOGICAL SATELLITE PROGRAMME

The two Earth satellites launched so far as part of the meteorological programme TIROS have already brought important results.

Tiros I (the name comes from Television and Infra-Red Observation Satellite) was placed in a 48·3° orbit that was nearly circular (466/428 miles) on April 1, 1960. During its seventy-eight days of operation (it is still aloft but not transmitting) it sent back nearly 23,000 pictures of the Earth and its cloud cover.

On November 23, 1960, *Tiros II* was placed in a 48·5° orbit that was slightly more elliptical (453/387 miles); it is still transmitting. This unit, like the first, contains a wide-angle and a narrow-angle television camera, infra-red horizon sensors and special solar cells to measure the unit's orientation, and the necessary power supplies and telemetry equipment. *Tiros II* contains, in addition, instruments for measuring solar and infra-red radiation in various spectral bands, and a magnetic device to control the orientation of the spin axis.

A third satellite, *Tiros III*, *is* scheduled for the second quarter of this year and *Tiros IV* is expected to go up early next year.

The first of the advanced weather satellites that are to follow—the *Nimbus* series—is scheduled for late 1962.

The two main objectives of this programme are, first, the observation of the cloud cover over the entire globe and, second, measurements which will show how the Sun's energy is converted into atmospheric motions. Later units of the *Nimbus* series are to be part of an operational weather observation system.

At the start, *Tiros I* produced two important findings. It disclosed the existence of (*a*) spiral cloud patterns with diameters as large as 1000 miles and (*b*) large areas composed of many crescent and doughnut-shaped cloud cells with diameters of thirty to fifty miles. Many of the large spiral cloud patterns were found to have a marked banded structure and were associated with cyclonic storms; no two were exactly alike. The smaller thirty- to fifty-mile wide cellular clouds were apparently of convective origin; these are usually too large to be seen as cells by a ground observer and too small to appear on conventional meteorological charts. These convective cells probably play an

important role in transferring heat, water vapour, and momentum from the surface to the lower layers of the atmosphere.

Tiros I photographs also revealed other interesting observations, including cloud bands of a tropical cyclone north of New Zealand, cirrus streamers that were associated with the jet stream over the southern Andes, double-vortex clouds in cyclones that apparently had only one low-pressure centre, cumulus cloud "streets" in the tropics, a single, long, thin cloud about five miles wide and several hundred miles long in the vicinity of the Gulf Stream east of Florida, and its reverse—a long, thin, cloudless streak embedded in an otherwise continuous cloud-cover near Madagascar. The photographs showed, in addition, a broken pattern of sea ice in the Gulf of St Lawrence and and snow-fields in the Himalayas and the Alps.

A series of photographs obtained from five orbits of *Tiros I* over the Southern Indian Ocean enabled construction of a synoptic cloud chart for that area that consisted of four main cyclonic centres; one of these storms later moved into southern Australia and produced precipitation over a prolonged period.

Tiros II is also producing good results; although the pictures from the wide-angle television camera are not as clear as expected, the narrow-angle camera and the infra-red sensors are giving good data. Shown here is a schematic drawing of the cloud-cover observations over the North Atlantic and part of Africa on December 1, 1960. This type of presentation was developed by the U.S. Weather Bureau to enable the transmission of cloud-cover information over national facsimile circuits.

There are two radiation experiments aboard this satellite. One consists of five radiation detectors oriented at 45° to the spin axis; they scan through a combination of the satellite's rotation and its movement along the orbit, measuring:

1. The Earth's albedo—the percentage of energy reflected from the Earth—in the spectral range 0·2 to 5 microns.

2. Infra-red radiation emitted by the Earth and atmosphere in the 7 to 30+ micron range.

3. Infra-red radiation emitted by the Earth in the 8 to 12 micron range. As the atmosphere is quite "transparent" in this band, this measurement should supply information that may enable (*a*) cloud detection at night (*b*) the determination of cloud-top temperatures and, accordingly, a rough measure of cloud-top height, and (*c*) the determination of surface temperatures where there is no cloud cover.

4. Radiation from the water-vapour spectral band—6·3 microns ±5%. This measurement should show the geographic distribution of the approximate upper margin of water vapour in the atmosphere, which may be near the height of the tropopause in some places—about 20,000 to 50,000 ft. above sea-level.

5. Visible radiation in the 0·5 to 0·7 micron range, to provide maps for use in relating the television pictures to the non-visible radiation maps.

The second radiation experiment consists of two sensors—one white and the other black—which together measure the heat balance of the parts of the Earth's surface and atmosphere that are viewed by the wide-angle television camera. The white sensor measures heat radiation from the Earth while the black one measures both the heat and visible radiation (reflected sunlight).

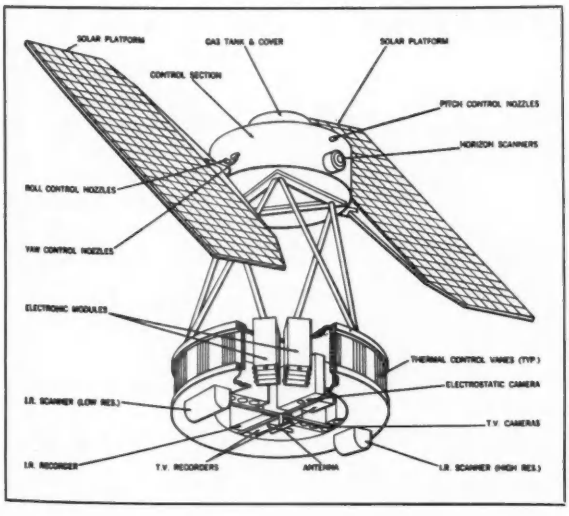
Data from the radiation experiments are recorded continuously on magnetic tape and played back on command from the ground stations.

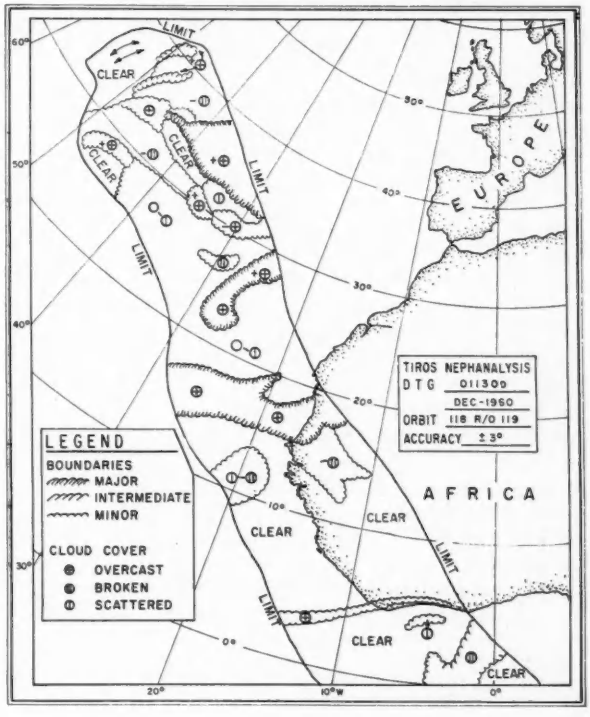
Orbital and instrumental details of *Tiros II* have been sent to all COSPAR participants (Committee on Space Research); data obtained will be deposited in World Data Centre A (the meteorological sub-centre at Asheville, North Carolina).

The first of the *Nimbus* satellites will weigh about 650 pounds (*Tiros* units are 280 pounds). They will be launched in near-polar orbits to give global coverage and will be oriented with gas jets and inertia wheels to keep the six television cameras on it pointed towards the Earth. The solar cells on two large paddles will keep the storage batteries charged.

(*Right*) Nephanalysis (cloud-analysis) of a portion of the 118th orbit of *Tiros II* on December 1, 1960. Plus and minus signs outside circles indicate relative density of cloud cover.
(From U.S. Weather Bureau)

(*Below*) Schematic drawing of *Nimbus* satellite.
(National Aeronautics and Space Administration, Washington, D.C.)





BIOLOGICAL CLOCKS

JANET E. HARKER

The 24-hour rhythms found in members of the plant and animal kingdom (including Man) continue to baffle investigators who find these clocks can be stopped or started but not slowed down or speeded up. One of the clocks in the cockroach has now been identified and isolated, throwing new light on the entire question.

Practically all modern biological research is directed towards answering questions about the mechanisms used by animals and plants in the performance of their many activities. It is rare indeed for any new physiological feature to be discovered, yet one has been found fairly recently that has been generally overlooked. It is the ability of plants and animals to measure time.

This discovery was preceded by many observations, over a long period of time, of the behaviour of animals in their natural environments. Many people noticed that an owl would hoot at the same time each night or that bats would begin to fly at the same time each evening. Careful recordings of the activity of medically important disease-carrying insects, mosquitoes, and biting flies clearly showed that each species in a particular locality was active at a specific time of day. Human beings, themselves, are conscious of an ability to measure time, many people finding they can wake up in the morning at a time decided upon the previous evening.

These observations by themselves do not show that animals can measure time, of course, as the apparent measurement of a time interval in every case may be a direct response by the animal to environmental variations in temperature, light intensity, humidity, noise or even atmospheric pressure. On the other hand, there are a number of records of the activity of birds in the northern Arctic which show that they rest during certain hours of the "night" during the summer months of continuous daylight, even though there seems to be little information from the environment as to whether it is "day" or "night".

Another indication of a time-sense which is not entirely the consequence of environmental variation can be seen in the behaviour of birds and other animals which navigate by the sun. Since the relative position of the sun changes during the course of the day, an animal navigating by the sun must allow for its relative movement and this implies an awareness of the passage of time.

We are left then with two hypotheses: one, that an organism's time-sense is simply a reflection of the changing environmental conditions over the course of 24 hours or, two, that there is some sort of "internal clock" which can be used like an alarm-clock to indicate when an activity should begin or, like a chronometer, which can be consulted at any time.

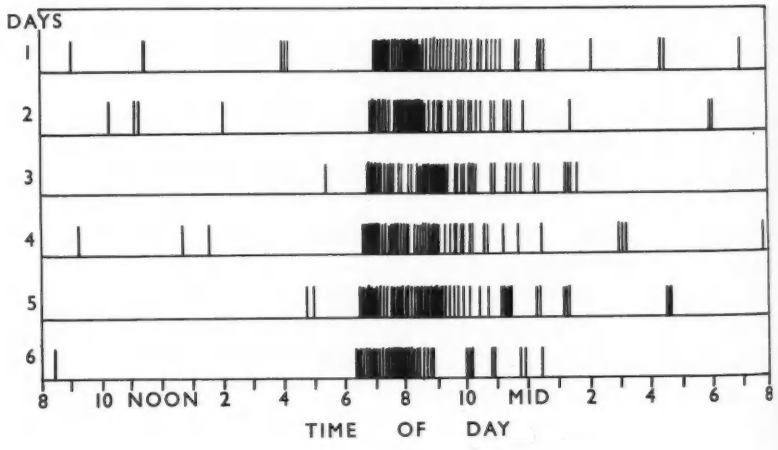
EXTERNAL TIME-SIGNALS OR INTERNAL CLOCKS?

The only way to test the two hypotheses is by subjecting the organisms to rigorously controlled environmental conditions in the laboratory. When this is done, it is found that animals as widely different as cockroaches and flying squirrels behave exactly as though they could see the sun setting in the outside world, even when they are kept in continuous darkness with the temperature, pressure, and humidity constant.

A recording of the activity of a cockroach living in a constant environment is shown in Fig. 1. Such records are made by shining a beam of infra-red light (invisible to the cockroach) across the animal's cage to a sensitive photo-electric cell so that the beam is interrupted whenever the cockroach moves. This "burglar-alarm" device automatically produces long-term records of the animal's activity outside the room in which it lives undisturbed. Fig. 1 shows that this particular cockroach was able to measure off time-intervals not of exactly 24 hours but of 23 hours and 53 minutes.

A slight deviation from 24 hours in an animal's rhythm is found to be quite common. Even two animals living in the same cage may measure off slightly different periods, so it seems unlikely that any unforeseen environmental variable is giving them a time-signal.

FIG. 1. Activity record of a cockroach placed in an unchanging environment.



Miss Harker, Ph.D., is a Demonstrator at Cambridge University and also a Fellow and Director of Studies at Girton College.

These experiments provide strong evidence for the existence of an "internal clock" which can act as an alarm-clock.

WHAT CAN A CLOCK DO?

The locomotor activity of an animal is, of course, only the outward and visible sign that the internal clock is running, just as the movement of the hands of a mechanical clock indicate it is going. There are many other physiological processes taking place in different organisms which show the existence of 24-hour clocks and can be used to give information about these timing mechanisms. Some, to name a few, are the number of red blood corpuscles in Man (Fig. 2), the flashing of luminescent protozoa, the movement of the leaves of a green plant (Fig. 3), the production of spores by a fungus, the emergence of a flying insect from the pupal stages or the colour changes in a prawn (Fig. 4).[1]

Although an "indicator process" recurs at 24-hour intervals, it is not immovably tied to any particular time of day. Just as the hands of a clock can be reset to show any time, so can an indicator process be reset. The stimulus which causes resetting is generally an environmental one. In natural conditions, the timing of an activity is frequently correlated with the changes between night and day; in controlled experiments, the onset of light or darkness is the major influence on the setting of the biological clock.

A cockroach runs about soon after it becomes dark. If the light in its cage is turned off at 9 a.m. and turned on at 5 p.m., the animal will, in a week or two, run about only during the day (the dark period inside his cage) instead of at night. If the cockroach is then transferred to continuous darkness, it goes on running about by day. The hands of its clock do not go back to their original position when the resetting stimulus is removed.

Yet another similarity between a biological clock and a mechanical clock is the fixity of its period. When animals are subjected to cycles of light and dark that alternate every 9 hours or 30 hours, for instance, the animals do not alter their 24-hour rhythm. In some cases, the rhythm is reset each time the light goes on or off giving the impression of

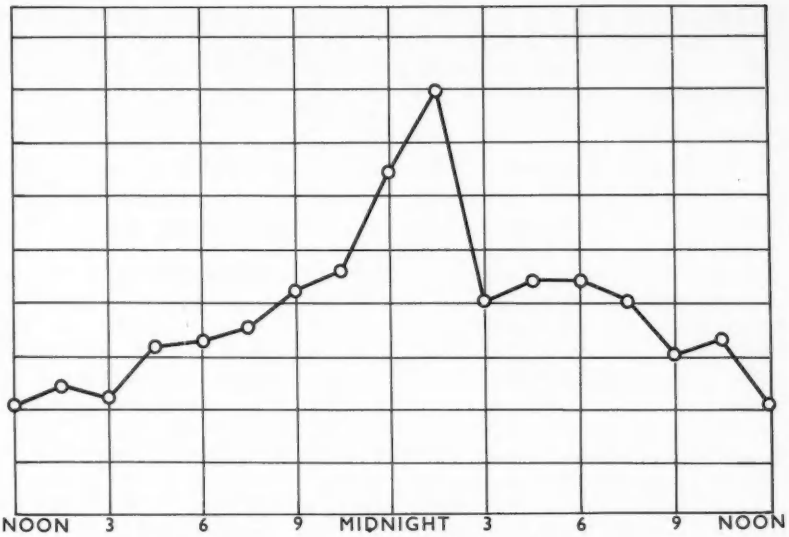


FIG. 2. Relative red blood corpuscle level in man over a 24-hour period. (Adapted from Halberg, Halberg, Barnum, and Bittner.)

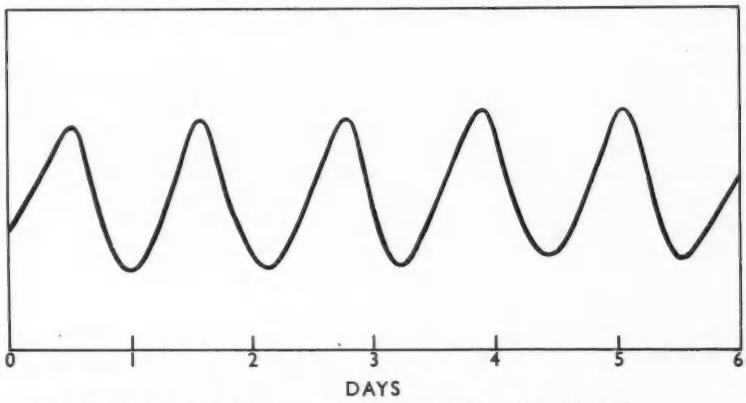
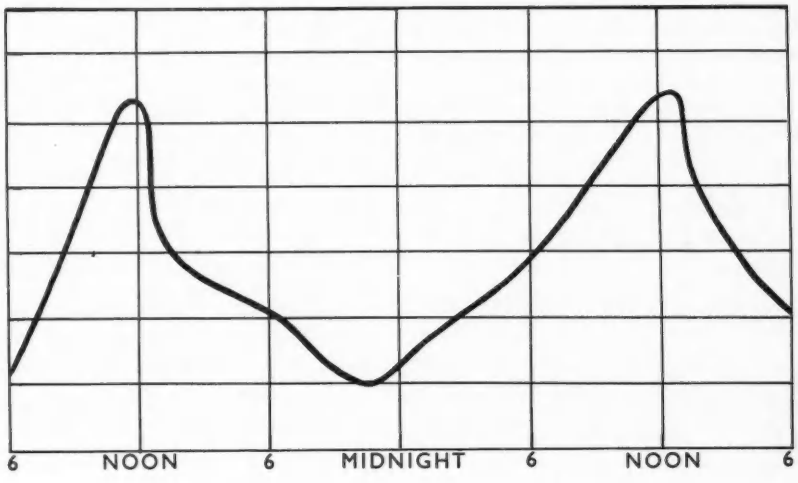


FIG. 3. Pattern traced by the bending of the leaves of the plant *Phaseolus* over a period of several days. (After Bunning.)

FIG. 4. Degree ofdispersion of the chromatophores on the leg of a prawn kept in continuous darkness for thirty-six hours. Contraction and expansion of the chromatophores cause a rhythmical colour change.



adaptation to the imposed light cycle, but the animal always returns to its 24-hour rhythm once the light cycle ceases. Its clock remains unchanged.

SENSITIVITY

To light: If an animal is kept in continuous darkness for a considerable period of time, its 24-hour rhythm disappears but a very small variation in the environment can cause its reappearance. Just how small this variation need be has been shown by experiments on the fruit fly *Drosophila.* Its rhythm can be initiated by a light flash lasting only 1/2000th of a second.[2]

Temperature: One of the most interesting characteristics of the internal clock is its insensitivity to temperature variations which lie within the normal biological range. In warm-blooded animals this is not unexpected since the animal itself maintains its metabolic processes at a controlled temperature. In cold-blooded animals, however (fish, frogs, snakes, and all invertebrates), the body temperature follows that of the environment and the rate of metabolism increases or decreases as the temperature rises or falls; yet, their clocks do not speed up or slow down. Temperature insensitivity is a requisite for a good time-keeper but how this apparent insensitivity is maintained in cold-blooded animals is not yet known.

Although a lowering of the temperature normally has no effect on time-measurement, the clock may stop suddenly at about 0°C. When the temperature is raised again, the clock restarts, but like any other clock which is restarted without resetting, it now runs late compared with a clock which has continued to run. When an animal is chilled for, say, 8 hours and then rewarmed, it begins to run about 8 hours later than usual.

Anaesthetics: Anaesthetics also stop some internal clocks but, under normal conditions, the clock is soon reset to a normal time by the usual environmental changes. Man, himself, does not run "slow" for a long period after an operation.

WHERE IS THE INTERNAL CLOCK?

All of these experiments have produced a good deal of information about the properties of biological clocks but very little indication of their whereabouts in organisms. Investigations were first directed towards the brain. The whole process of estimating time was first thought to be one of learning, but this theory was upset by the type of experiment already mentioned in which one brief flash of light initiates a rhythm in animals which for several generations have shown no rhythm. One flash of light gives the animal no information on the length of a 24-hour time-span.

Various parts of the brain have since been considered possible centres for the clock system, and exploratory operations have been made on a great variety of animals. The removal of parts of the brain have resulted in the stopping of some rhythms but others have continued, indicating that the part of the brain removed may have controlled the functioning of that part of the body but not necessarily its rhythm. No one region of the brain has ever been found to control the entire time-sense of an organism. In assessing the results of these and other experiments, a difficulty arises because we do not know whether there is just one clock controlling all the rhythms or several clocks which function collectively or individually.

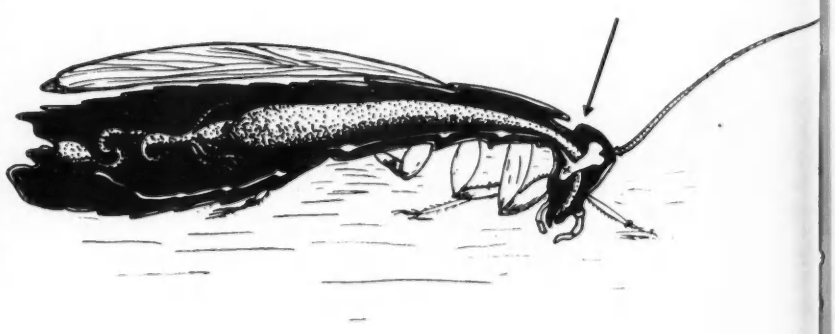


FIG. 5. A drawing of the inside of a cockroach showing the position of the nerve ganglion (*marked X, arrow*) which contains the neurosecretory clock.

ONE CLOCK IDENTIFIED

One of the clocks in the cockroach has been identified. It is a clock which measures time in periods of 24 hours and continues to do so when it is out of contact with any of the cells of the parent animal.

My discovery of this clock followed experiments in which the blood systems of pairs of cockroaches were joined together to give a Siamese twin effect. Since more than one physiological rhythm in an animal can be reset by the same environmental stimulus, it seemed likely that a hormonal link might exist between the clock (or clocks) and the indicator processes. If this were so, the presence of a hormone in the blood of one animal might affect the setting of the rhythms in another animal.

One cockroach of each pair used in the experiment was kept in constant light all its life and showed no sign of an activity rhythm. The other was kept in a 12-hour-light:12-hour-darkness cycle and showed a clear rhythm of activity. The two were then joined back-to-back so that the walking member of the pair was the arhythmic one, carrying the other with it. After the union, each pair was kept in continuous light. Records of their activity showed that the mobile cockroach began following a 24-hour rhythm. The greatest point of interest was the observation that the time of activity—the setting of the hands of the clock as it were—was correlated with the time at which the upper cockroach had previously been active. From these results, it seemed fairly clear that a hormone was involved in the rhythm.[3]

The next step was the search for the source of the hormone. After many false starts, it was eventually traced to four specialised cells found in a large nerve ganglion just under the oesophagus (Fig. 5). They are of a type known as neurosecretory cells—that is, nerve cells

which are specialised to secrete hormones (Fig. 6).

At this stage, there was no evidence that the neurosecretory cells were a clock, but it was soon found that the cells could be transplanted into the blood-stream of another animal and there maintain their secretory rhythm, proving that the cells could act as an independent timing mechanism.[4] This did not tell us anything about how they were doing it, however.

With one clock isolated, there now seemed to be some chance of finding out whether there were others in the cockroach. What we hoped to do was stop the neurosecretory clock, let the others (if any) continue to run, and then restart the neurosecretory clock so that it was out of phase with the rest

FIG. 6. One of the cockroach's large neurosecretory cells and its axon (*stained, arrow*).

of the animal. We eventually found a way of doing this with a device which controlled the temperature of the neurosecretory cells inside the animal without affecting the temperature of the surrounding tissue.[5] When the cells were chilled to 0°C, the clock stopped. This was proved by letting the clock warm up again, removing it from the animal and measuring its secretory rhythm inside the blood-stream of another animal; the time of secretion was "late" by the number of hours for which the cells had been chilled. But what did we find when the cells were not removed from the animal after chilling? When the chilling was not continued for more than a few hours, the clock did not seem to be affected at all; secretion took place at the usual time.

The discrepancy between these two results could only mean that some other clock had continued to run in the warm part of the animal while the cells were being chilled and that this had acted as a master clock and reset the neurosecretory rhythm.[6]

The presence of two clocks controlling one rhythm may seem both unlikely and unnecessary—particularly since each appears capable of keeping accurate time—but the use of two clocks is apparent when the animal is considered in its natural habitat. The master clock is very sensitive to variations in light intensity—an animal is likely to experience such changes when it crawls under bark or undergrowth or when clouds pass over the moon. If the whole activity rhythm were to be reset each time the light altered, the function of the clock would be very limited.

The activity rhythm is, however, under the control of the neurosecretory clock and this can only be reset by the master clock when the two are only slightly out of time with each other. This means that as the days lengthen or shorten with the seasons of the year, the master clock will be reset and will, in turn, reset the neurosecretory clock and the onset of activity; on the other hand, if the light intensity alters at unusual times of the day or night, the animal's rhythm is not affected.

The story of the activity rhythm does not stop there. During the past year, it has been found that the production of the neurosecretory hormone is dependent on a second hormone, and there is evidence that it, too, is secreted rhythmically, though not in phase with the first.

This side of the work on biological clocks has its practical difficulties. In order to find out whether the nerve axons that are believed to carry the second hormone really do so, it was necessary to block the flow along the axon by means of a ligature. The nerve is extremely small—about 0·01 mm. across in the relevant part—and the finding of a thread small enough to tie around it became a major problem. The cockroach itself finally provided the solution—it was found that the extremely thin spiral lining of the insect's breathing organs (the tracheae) could be drawn out as a fine thread, persuaded around the nerve, and tied.[7]

The discovery of the second hormone is of interest because it suggests that separate clocks may not be autonomous. We may find there is an almost imperceptible grading between tissues which are acting as clocks and those which are not. It may indeed be that practically all cells can act as clocks in some circumstances.

UPSETTING THE CLOCK

So many physiological processes appear to be controlled by internal clocks it is obviously of great interest to know what happens to an animal if anything goes wrong with the controlling system. Until recently, it has not been possible to investigate this problem, simply because there was no known way of upsetting the clock; environmental stimuli can reset the clock, but they do not affect the periodicity.

With the discovery of a neurosecretory clock which can maintain its periodicity within another animal, however, it was possible to expose animals to two clocks running out of time with each other. This was achieved by keeping one set of cockroaches in light during the night and in darkness during the day until their clocks were reset to run twelve hours out of phase with those of animals living in normal conditions. The clocks from the reversed day animals were then implanted into normal cockroaches. With two clocks controlling their activity rhythms—each acting at different times of day—all the cockroaches developed malignant tumours (cancer), most of them appearing in the intestine.

Since the particular clock we have been dealing with secretes a hormone, the abnormal concentration provided by two clocks may be involved in the tumour formation, particularly in view of the known hormonal involvement in other types of tumours. Control animals implanted with clocks which were running in time with their own clocks did not develop tumours, despite the fact that they were exposed to abnormally high hormone concentrations.

A great deal remains to be discovered in this field. Further work may produce important and far-reaching results but any interpretations made at the moment would be pure speculation.

INTERNAL CLOCKS AND MAN

The timing mechanism in the cockroach may, at first glance, appear to have little connexion with man, but investigations of the internal clocks in the animal and plant kingdoms have shown they all have properties in common. Thus, whatever we can discover about any of these clocks may bring us closer to an understanding of the mechanism as a whole.

A large number of records have been made of 24-hour rhythms in man, ranging through a wide variety of physiological processes and including rhythms of body temperature, mitosis, numbers of blood-cells, substances excreted, liver metabolism, and hormone concentration. There seems to be no doubt about the existence of biological clocks in the human system.

Experiments with other animals have shown that normal 24-hour rhythms will not follow environmental cycles which depart far from a 24-hour period, nor will a rhythm be rapidly reset in a new environmental cycle if this departs widely from the previous cycle. Whether man shows a similar lack of accommodation in his response to cycles of an abnormal period and slow resetting in inverted light-dark cycles are clearly important questions. Workers who change shift at frequent intervals might have their physiological rhythms constantly out of phase with their sleep-work cycle. Travellers flying long distances, particularly when zigzagging east and west, are subjected not only to reversals of environmental cycles but also to cycles that vary in length from the normal 24-hour period.

There is already some information about these problems. Studies have been made of the excretory rhythms of shift workers, and carefully controlled experiments have been run on isolated groups of people living in the continuous daylight of the Arctic summer. In both cases, reversal of the sleep-activity routine was followed by a reversal in the excretory rhythms, but it was a matter of days before the rhythms came into phase; in some instances, it took several weeks.[8] One interesting experiment has shown that a hormonal rhythm takes up to nine days to adapt to the time change involved in a flight from America to Japan.

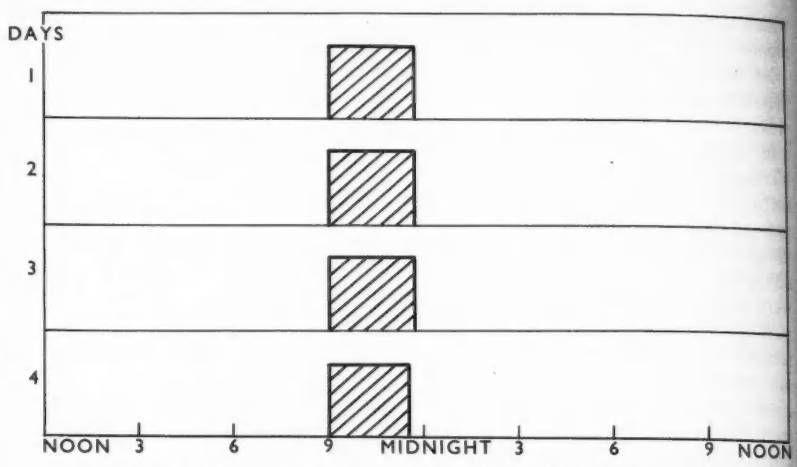


FIG. 7. Record of a woman suffering from Parkinsonism, the shaded areas representing the times at which she was able to feed, walk, and talk normally. (After Richter.)

Some people adapt to new conditions much more easily and rapidly than others; the slow adapters undergo considerable stress when they are placed in an environment that is completely out of phase with their natural rhythm.

In strange contra-distinction to the fact that stress follows the upset of normal physiological rhythms, there stands the fact that some illnesses appear to emphasize or initiate 24-hour rhythms which are not normally measurable. Some of these effects are made apparent through mental or emotional symptoms. Richter[9] quotes the case of a patient who, over a nine-year period, was bedridden, unable to speak clearly, walk, or feed herself until 9 p.m. At that hour she underwent a sudden change in personality and was able to walk about, talk clearly, and look after herself (see Fig. 7).

The recurrence of physical symptoms at strict 24-hour intervals has also been recorded, sometimes for as long as thirty years, despite the many internal and external environmental variations experienced during that period. Richter suggests that there are a number of clocks in the body which are not normally running in phase with each other but which, under the abnormal conditions of some illnesses, are brought into phase to produce clearly defined physical or mental cycles.

Only the alarm-clock type of internal clock has been discussed here in any detail, but the chronometer type shows the same properties. It seems highly probable that the two are identical but the problems raised by either are still far from being solved. We do not yet know how many clocks there are or how independent they are. Their cellular mechanism remains the most baffling problem.

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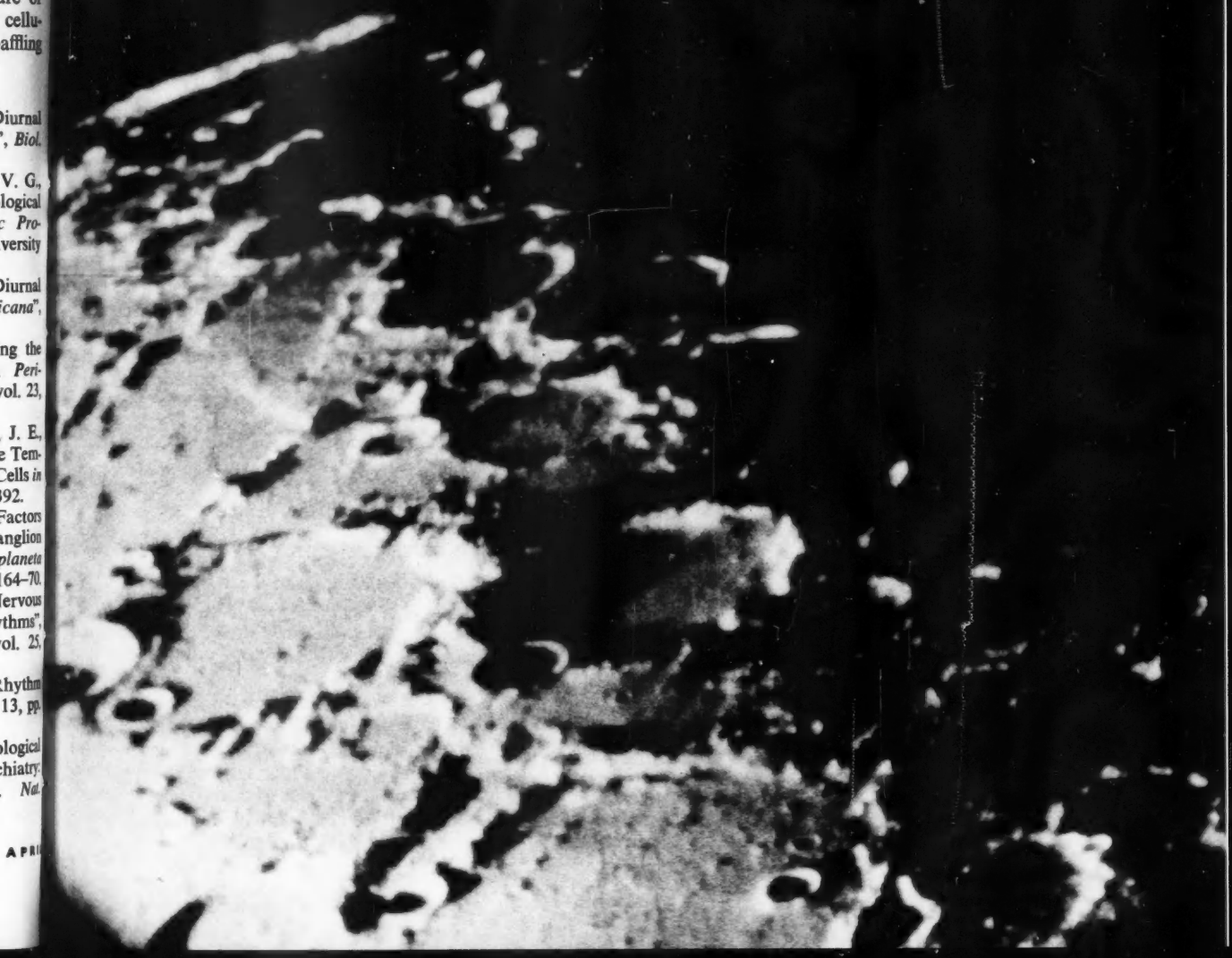
WATER ON THE MOON?

ZDENĚK KOPAL

A re-examination of this question indicates the generally accepted answer may be wrong. A closer study of the surface, particularly near the poles (one region shown here), is now in order.



This article has been given a deliberately provocative title to make us stop and give some serious thought to a question which, to the lay mind as well as to many an expert, would appear to have been answered quite emphatically in the negative. For haven't we heard since childhood that the Moon is a bone-dry globe on which no water can remain on account of the absence of any atmosphere? Do we not recall that Galileo Galilei and other early telescopic observers have been castigated by posterity for having misnamed the lunar flat-lands as "seas"? And do not at least the experts remember the scorn with which the astronomers received Hörbiger's lunar "ice-hypothesis" only a generation or two ago?

The aim of this article is not to present any apologies for the aberrant notions of our predecessors; their ideas could not even have been justified by valid arguments when they were put forward. There is, however, seldom any finality in scientific judgments pronounced by any generation; as science advances, the ghosts of old problems often take vindictive delight in coming back from the shadowy obscurity to which they may have been temporarily exorcised by our predecessors. New generations armed with new knowledge may see them in different light. This, I submit, has now happened with the age-old question of water on the Moon. The aim of this paper will be to state some of the reasons.

ORIGIN AND FATE OF THE JUVENILE WATER

To begin at the beginning, let us retrace our steps all the way back to the time of the origin of the solar system, some 4600 million years ago (the date being set essentially by the study of the ages of the meteorites incident on the Earth). Following the now discredited "catastrophic" theories of the origin of the solar system from hot matter derived from the solar interior, the view prevalent among students of the subject at present is that the planets and other solid bodies of the solar system have grown by an accumulation of smaller pre-existent particles and dust in a solid state, and at moderate temperatures (of the order of, at the most, a few hundred degrees Kelvin) simultaneously with the birth of our central luminary. If this view is correct—and it has certainly much to commend itself to our attention—then it is very probable that the chemical composition of the primordial gas-and-dust cloud which eventually collapsed into the solar system must have been very similar to the chemical composition of the solid meteoritic debris still floating in space around us, those left-overs from the days of the formation of the solar system which our Earth keeps sweeping in from space on its perpetual journey round the Sun. Their chemical analysis reveals that they contain (apart from atoms and molecules of many light common elements) non-vanishing traces of certain volatile compounds, the most important of which is water; in chondritic meteorites, it is found to be present in amounts of the order of 0·1%.

The presence of water in primordial cosmic matter should cause no surprise in view of the high cosmic abundance of its constituent elements and the great stability of its molecule. What interests us more directly, however, is the role (and subsequent fate) of juvenile water which was present during the formation of bodies of lunar or planetary size. If such bodies were born at temperatures in excess of 1000°K, most of it would have been driven out at the beginning and (for masses as small as that of the Moon, which are unable to exert enough attraction) irretrievably lost into space. If, on the other hand, the original temperature of such bodies did not—as we believe—exceed a few hundred degrees, the juvenile water would have remained bound in the crystal lattice of solid hydrates.

What did happen to this water (and other volatile compounds) since the formation of the Moon as a body we know today? The answers depends critically on subsequent temperature developments inside the Moon, governed by the radioactive heating of its interior from the spontaneous decay of such elements as should occur in the bulk of the Moon's mass—by analogy with the Earth's crust or chondritic meteorites—in significant amounts (in particular, potassium-40, thorium-228, uranium-235, and uranium-238). The outward flow of this heat has undoubtedly been greatly impeded by the low thermal conductivity of the lunar rocks. The outcome of extensive detailed studies of the balance of lunar heat transfer, particularly by Urey, leaves no room for doubt that the Moon must be warming up, and that the bulk of its mass is at present maintained at a temperature in excess of 1000° to 1200°K, sufficient to expel water molecules from the crystal lattice of most solid hydrates and drive them under their own pressure (in the form of superheated steam) along any crack or fissure opening outwards.

• Unless the foregoing argument contains some fatal flaw that is not apparent at present, there is but little room for doubt that most of the deep interior of the Moon (with the temperature in excess of 1000°K) must now be rather completely desiccated, and its outer crust correspondingly enriched with water. But how close can this water get to the surface?

The surface of the Moon—as we know from measurements of its thermal emission in the domain of radio-frequencies—is quite cold; at a depth of less than one yard (to which the diurnal heat-wave can no longer penetrate) a well-nigh constant temperature of 230° ± 10°K (some 40°C below the freezing-point of water) is encountered all over the Moon. The hot steam slowly seeping outwards through the lunar crust must therefore have condensed into a liquid and frozen into ice long before it reached the surface. Along crustal fissures which can serve as ducts for more rapid escape, the formation of ice may, in fact, have taken place very close to the surface and given rise to morphological phenomena which might possibly reveal the presence of sub-surface lunar glaciers to the external observer from Earth.

THEORY SUPPORTED BY OBSERVATIONS AND CALCULATIONS

At the recent Leningrad symposium on the Moon, Gold pointed out that such sub-surface lunar glaciers (covered by dust and other surface debris) are observed by us as the so-called lunar "domes"—minor bulges found in great numbers on the visible lunar surface, and usually in clusters. The largest of such bulges (found in the neighbour-

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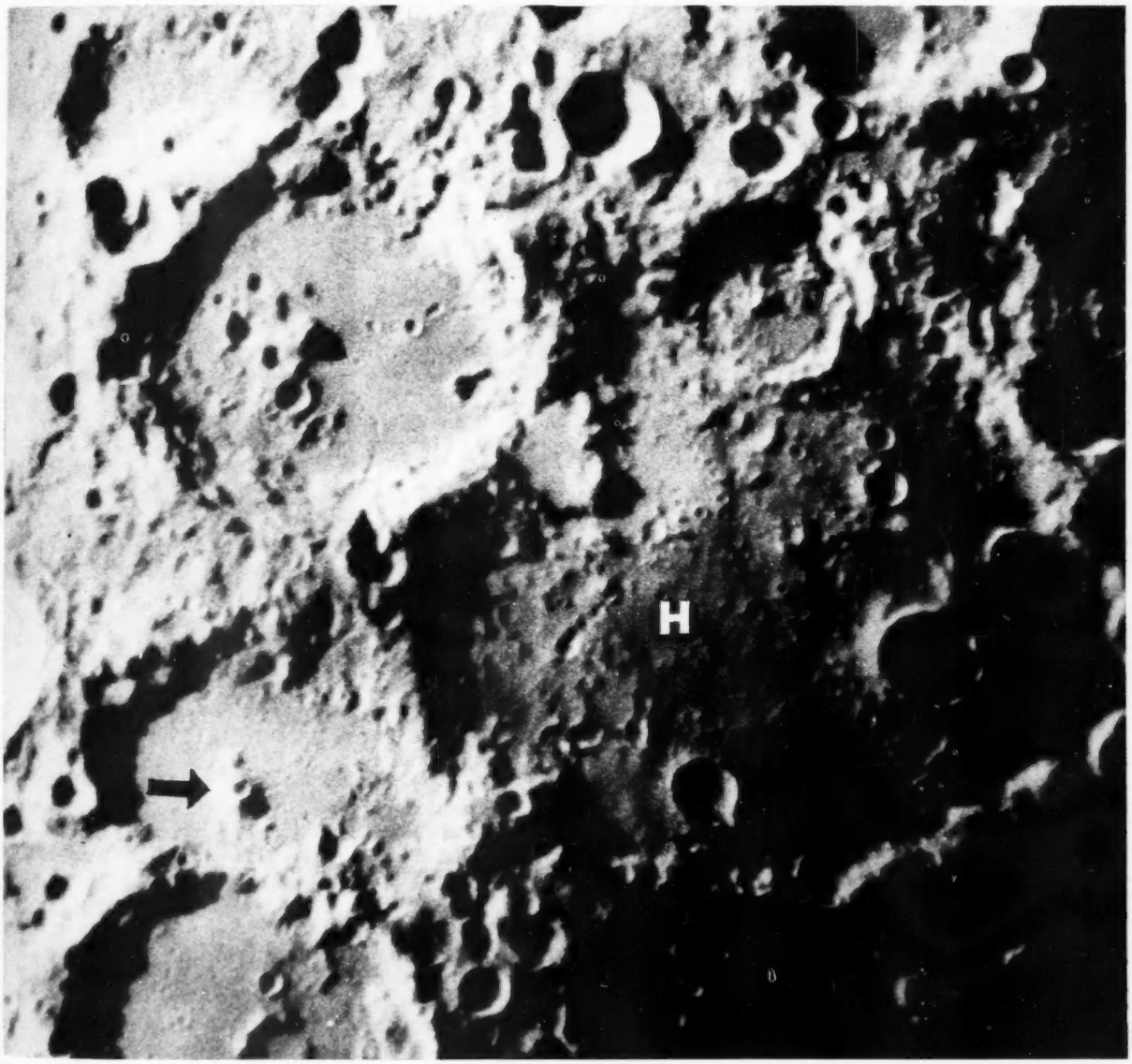
hood of the lunar crater Copernicus) are 20 to 25 km. across, but the majority of them are 3 to 6 km. across and 100 to 300 m. high. The photograph of the Moon shown below reveals a typical cluster of lunar domes of this size inside the walled plain marked *H*; there are at least thirty-five such formations over an area of a few hundred square miles. These domes bear more than a superficial resemblance to the "hydro-lacoliths" in Alaska and northern Canada which may indeed represent their terrestrial homologues. Closer investigation will be necessary before such a suggestion can be placed on a more secure basis, however.

Let us suppose, as an extreme case, that juvenile liquid water escaping from the hot interior of the Moon reached the surface in occasional spurts—as geysers. Such phenomena have not actually been observed so far, to be sure (though they would be very difficult to detect except under very special circumstances), but some of the largest lunar domes referred to above show some evidence of central depressions which may indeed represent possible springs of such geysers. If hot water occasionally spurts through them into space, what happens to it afterwards?

The exact answer depends, of course,

Region of the Hell plain (marked *H*) near the centre of the apparent disc of the Moon showing a conspicuous clustering of small domes within its enclosure. To the left, an arrow indicates the position of the "hill-top" crater Regiomontanus A (almost a replica in size and shape of the terrestrial volcano Krakatoa) which undoubtedly represents another product of lunar desiccation and degassing processes.
(Photo: Pic-du-Midi Observatory, Manchester Lunar Programme)



on the actual temperature of the geyser and the altitude it might attain, but there is little doubt that if there were such an outburst during the lunar night or in places shielded from direct sunshine, a good part of the squirting water would not evaporate but condense into ice. What, then, would be the mean lifetime of such ice, exposed directly to the conditions of inter-planetary space without the protection of an atmosphere?

It so happens that this lifetime would be surprisingly long. This most important fact has been largely overlooked by our astronomical predecessors, although the basic knowledge was at their disposal from terrestrial laboratories for some time. Thermo-electric measurements of the heat radiation received from the Moon have disclosed that the prevailing temperature on that part of the surface which is shielded from direct sunlight is around 120°K—not much above the temperature of liquid air. At this temperature, the rate of evaporation of ice is known to be approximately $3{\cdot}1\times10^{-14}$ gm./cm.²/sec., giving rise to a vapour pressure above it of only $1{\cdot}4\times10^{-12}$ mm. of mercury. The latter figure represents, then, the maximum pressure of water vapour which could exist over the lunar surface except for brief periods around high noon and is many orders of magnitude less than the corresponding pressures for SO_2 or the rare gases.

This rate of evaporation of ice would correspond to a loss of only $4{\cdot}6\times10^{3}$ gm./cm.² during the entire age of the Moon. In other words, if the Moon had been surrounded by a permanent shell of ice since the days of its formation, less than 50 m. of its outer crust would have been lost by sublimation during the past 4·5 thousand million years! The glaciers of our terrestrial Antarctica are several kilometres deep.

What is the maximum depth of glaciers which could have conceivably been formed on the Moon? To make an estimate, let us draw an analogy with the Earth. If the total amount of water in all of the oceans of the Earth were uniformly distributed over its surface, it would cover the entire globe with a uniform layer about 1800 m. deep. If all this water had actually been produced by the desiccation of the entire mass of the Earth (which is equal to $5{\cdot}98\times10^{27}$ gm.), each gram of this mass would have been forced to contribute $1{\cdot}54\times10^{-4}$ gm. of juvenile water. If we assume, now, that this figure can be applied to the Moon, its smaller mass of $7{\cdot}35\times10^{25}$ gm. should have liberated an amount of juvenile water which would have covered the lunar globe with an ocean some 300 m. deep if squeezed out completely to the surface. If frozen, it would have produced approximately 3×10^{4} gm. of ice/cm.² This amount is about ten times as large as that which would be lost by sublimation (at night conditions) during the entire age of the Moon.

These figures are, of course, highly approximate, for they assume a total escape of all the juvenile water to the surface of the Moon (a process which may still be far from being complete) and its uniform distribution. In actual fact, its efflux to the surface may have occurred in limited localities, leaving the major part of the ground dry. Watson, Murray, and Harrison Brown have recently pointed out that the foregoing arguments (due largely to them) should make it both hopeful and profitable to look for the presence of exposed ice in the permanently shaded areas of the lunar globe near the poles (see p. 143) where the Sun never rises totally above the horizon, and where low nighttime temperatures prevail most of the time.

What the results of such a search may be, only the future can tell, but it is becoming increasingly obvious that our former views concerning the role of the water on the Moon are due for drastic revision.

SUMMARY

Let us recapitulate the salient points of the arguments in favour of such a change. If the Moon originated at essentially low temperatures (not more than a few hundred degrees above absolute zero) by a condensation of solid particles chemically akin to the chondritic meteorites, its total mass of $7{\cdot}35\times10^{25}$ gm. must have contained $1{\cdot}13\times10^{22}$ gm. or 2·7 million cu. miles of juvenile water. If, in addition, the mass of the Moon initially contained an appropriate proportion of radioactive elements, the heat arising from their spontaneous decay (coupled with the low thermal conductivity of the lunar globe) must have raised the internal temperature of the bulk of lunar mass above 1000°K, and led to its large-scale desiccation. The water expelled from the hydrates by the rising temperature must, in turn, have found its way outwards gradually, enriching the water content of the outer crust of the Moon considerably.

Some of the accompanying subterranean processes may have left visible landmarks on the lunar surface and a fraction of the juvenile water may even have made its way to the surface and frozen there—especially in the polar regions. The mean lifetime of ice under night conditions on the Moon proves, moreover, to be so long that the theoretical likelihood of its presence in favourable areas appears to be considerable, though none has been found by direct observation so far.

Let us examine how all this affects our present picture of the Moon. We must still regard its surface as bone-dry, for liquid water must be completely absent from it at all times (except, under the rarest of conditions, during possible subterranean geyser eruptions). Liquid water must also continue to be discounted as an eroding or transforming agent of any importance. The term "maria" continues to be a misnomer for the arid lunar flat-lands. (Even a piece of ice supercooled to −40°C would feel like pretty dry stuff.)

On the other hand, the subterranean processes accompanying desiccation and the slow but large-scale motions of juvenile water (or of other volatile compounds) beneath the surface of the Moon may be very significant and may have had a much greater hand in the formation of many morphological characteristics of the lunar landscape (besides the domes) than has so far been thought likely.

Last but not least, the possibility of finding frozen water on (or near) the surface of the Moon should be of great and vital interest to the intrepid astronauts who may be stepping out of their space-ships on lunar ground some time in the next decade. The availability of water may even spell the difference between life and death for them. The terrestrial astronomers and physicists owe the explorers of the future all the data and information on this subject that can be secured in advance.

GENERATION OF ELECTRICITY IN THUNDERCLOUDS

B. J. MASON

The mechanism by which lightning is produced in thunderclouds has never been satisfactorily explained. Recent laboratory experiments have now led to a theory which accounts, quantitatively as well as qualitatively, for this elusive phenomenon.

Ever since Benjamin Franklin and the French physicist d'Alibard showed in 1752 that electricity was present in thunderclouds like that shown in Fig. 1, its origin and manifestation as lightning have provided challenging scientific problems which still await solution.

In essence, the thunderstorm may be regarded as an electrostatic generator which produces positive and negative electrical charges, the positive charge becoming concentrated in one region of the cloud and the negative charge becoming concentrated in another. As separation of the positive and negative charges proceeds, the voltage difference between the two oppositely charged regions (or between one of them and the earth) grows until electrical breakdown of the air occurs. At least part of the separated charge disappears during the passage of the lightning flash. The charge then builds up again and the same sequence of events is repeated.

In principle, the operation of this "natural dynamo" is simple, but the detailed mechanisms of charge generation, separation, and dissipation are very complex. Although there has been no shortage of theories, none has withstood the test of time and the accumulation of detailed information concerning the structure and behaviour of thunderstorms. A satisfactory theory must explain *quantitatively* how electric charge is generated and separated at a sufficient rate to account for its dissipation in lightning flashes. It must account for the observed distribution of positive and negative charges in the thundercloud and the observed changes in the electric field produced by the lightning flash. The theory must also be consistent with the size and duration of a storm as well as the scale and intensity of the rain and hail which accompany lightning activity.

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ESSENTIAL CHARACTERISTICS OF A THUNDERSTORM

As C. T. R. Wilson demonstrated thirty years ago, the negative charge of the thunderstorm is centred in the lower part of the cloud, where the temperature is about −5°C; the main positive charge is situated several kilometres higher up, where the temperature is usually below −20°C. Violent electrical activity usually occurs simultaneously with heavy precipitation (rain or hail). The first lightning flash occurs about ten to twenty minutes after the onset of precipitation inside the cloud (as detected by radar—Fig. 2).

Most theories of charge generation assume that precipitation plays an important role. Since the main charge centres appear at temperatures below 0°C, it is natural to associate their generation with the growth of ice particles. From observations made inside thunderclouds capping the Zugspitze, Kuettner has reported that the onset of lightning coincided with the appearance of solid precipitation—pellets of soft hail. Furthermore, sudden sharp increases in the electrical field with the appearance of soft hail pellets have recently been recorded by aircraft attached to the Cloud Physics Project of the University of Chicago.

The average electrical moment (charge × separation) destroyed in a lightning flash is about 100 coulomb/km.,

(*R. S. Scorer*)

FIG. 1. A thundercloud composed of a mass of ice crystals.

the loss in charge amounting to 20 to 30 coulombs. The manner in which the electric field recovers after the passage of the first lightning flash indicates that the quantity of charge generated and undergoing separation is about 1000 coulombs. In a typical thunderstorm of modest size, this charge is generated and the charge centres are formed between the 0°C to −40°C levels in a volume of about 50 cu. km. (a radius of about 2 km.). This action must take place within the ten- to twenty-minute period between the appearance of precipitation that is detectable by radar and the first lightning flash. The generation of 1000 coulombs of charge in a volume of 50 cu. km. in about twenty minutes means the average rate of generation is about 1 C/km.^{3}-min.

The more important of the existing theories were reviewed in the light of this requirement in earlier articles, and were shown to be incapable of producing charge at anything like this rate and/or were shown to be incompatible with the observed structure and behaviour of thunderstorms.

SEARCH FOR A NEW THEORY

In searching for a new mechanism, I have been particularly impressed by the following clues:

1. The main charging mechanism is probably associated with precipitation —most likely soft hail formed by the impaction and freezing of supercooled cloud droplets. There is increasing evidence that the appearance of soft hail in a cloud is accompanied by strong electric fields and lightning activity.

2. Laboratory experiments performed by W. Findeisen in Germany during the last war and repeated by other workers since have shown that when supercooled droplets are sprayed on a cold surface to produce a layer of rime ice, the rime acquires a charge; the air also acquires a charge but of opposite sign. Although opinion was not unanimous, most of the workers reported the charge acquired by the rime was negative.

3. In some recent experiments, J. Maybank and I have found that the freezing of water droplets with a radius of 0·03 to 1·0 mm. is often accompanied by the bursting of the ice shell (see Figs. 3 and 4) and the ejection of small splinters of ice carrying a positive charge, leaving negative charges on the droplet residues. This suggests that the formation of pellets of soft hail in a cloud by the impaction and freezing of supercooled drops will lead to the formation of negatively charged hailstones which will fall towards the base of the cloud while the positively charged ice-splinters are carried by the up-currents towards the top of the cloud, as shown in Fig. 5.

A rather different mechanism has been suggested by Reynolds, Brook, and their co-workers in the United States. They have some laboratory evidence that a hail pellet can be given a charge by the impact and bounce-off of small ice crystals having a slightly different temperature. They found that a slightly colder crystal bounces off with a positive charge, leaving the hailstone negatively charged, while a reversal of the temperature-differential produced a reversal of signs. Under natural conditions, one would expect the surface of a hailstone to be slightly warmer than the ice crystals because of the latent heat released by the droplets freezing on the hailstone; the hailstone would thus be expected to acquire a negative charge.

The electrification of hailstones caused by both the splintering of freezing droplets and collisions with ice

FIG. 2. Radar echoes from a very large thunderstorm. The main echo comes from two large cells, one of which extends to a height of 42,000 ft. Small cells in an early state of development are seen on the right. Horizontal lines mark intervals of 5000 ft.

(*By courtesy of the Director-General of the Meteorological Office*)

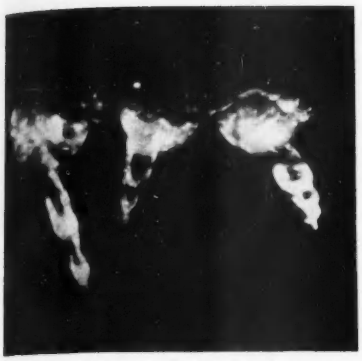


FIG. 3. Photograph showing the appearance of large spikes in the ice shells of freezing water drops. The shell is ruptured by the expanding water inside, the water ejected then freezing into spikes of this kind.

crystals has recently been the subject of a detailed study by J. Latham and I. The results of these experiments together with a new theory of the mechanism will now be described.

BASIC MECHANISM OF CHARGE SEPARATION

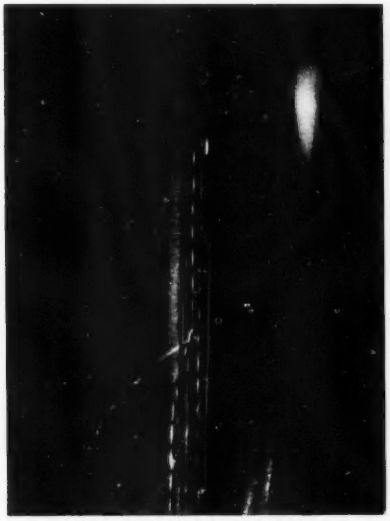
The fundamental problem is to explain how the electric charge can be separated during the bursting of a freezing drop or during a temporary contact between two pieces of ice at different temperatures. In both cases, the existence of a temperature gradient is involved and the new theory discusses how the positive hydrogen ions (H^+) and the negative hydroxyl ions (OH^-)—formed by the dissociation of a small fraction of the ice molecules—become separated under the influence of this temperature gradient.

The process depends essentially upon the fact that the concentrations of both positive and negative ions increase quite rapidly with increasing temperature, and the fact that the positive H^+ ion diffuses much more rapidly through the ice crystal than does the negative OH^- ion. Thus, if we imagine a steady temperature difference maintained across a piece of ice, the warmer end will initially possess higher concentrations of both positive and negative ions. The more rapid diffusion of H^+ ions down this concentration gradient will lead to a separation of charge with a net excess of positive charge in the colder part of the ice. The theory indicates that a steady temperature difference of $\triangle T$ (°C) will be accompanied by a potential difference of about $2\triangle T$ (millivolts) across the ice.

Such potential differences have been measured experimentally across specimens of pure ice and it has been shown that they are not radically affected by the presence of impurities such as dissolved salts and gases in the ice.

Thus, when two pieces of ice at different temperatures are brought into temporary contact and separated, the warmer piece should acquire a negative charge and the colder one should acquire an equal positive charge. The

FIG. 4. Photograph showing the ejection of tiny ice splinters from a small droplet (0·1 mm. in diameter) which has just frozen while falling through a column of cold air.



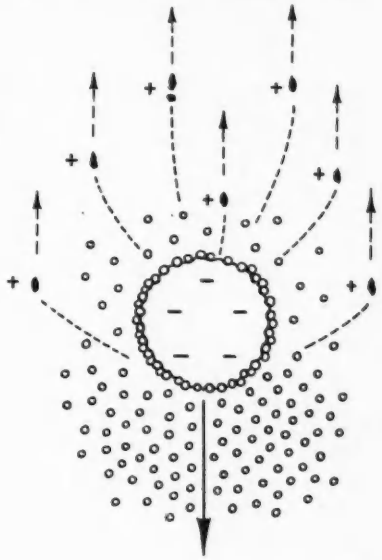
theory indicates that a maximum charge transfer of $3\times10^{-3}\triangle T$ e.s.u. (electrostatic units) should appear on each square centimetre of the contacting surfaces when these are separated after about 0·01 sec. If they are left in contact for longer times, the charge separation will be decreased as the two pieces of ice become more nearly equal in temperature.

These conclusions have been confirmed by experimental measurements which show that very little charge separation occurs if the contact period exceeds 0·5 sec. Experiments in which the ice was contaminated with carbon dioxide, hydrofluoric acid, and sodium chloride in concentrations up to fifty times that normally present in rainwater showed that the electrification was not greatly influenced by these impurities.

The preferential migration of protons (H^+ ions) down a temperature gradient explains why an ice crystal making temporary contact with a warmer hailstone will leave the latter with a negative charge. The electrification of freezing water droplets can also be explained by this mechanism, as follows:

During the early stage of freezing, a shell of ice is formed around the outside of the drop and the liquid interior proceeds to freeze at a rate determined by the rate of dissipation of the latent heat of fusion to the surroundings. During this process, a radial temperature gradient is established across the ice shell, the inner surface being held at 0°C by the water that is still liquid inside and the outer surface cooling towards the lower air temperature. According to the above theory, protons will migrate preferentially down this temperature gradient and produce a positive charge in the outer layers of the ice. When the droplet bursts by the expansion of the freezing centre, splinters ejected from the outer layer will tend to carry away a positive charge and leave the remainder negatively charged, as observed.

FIG. 5. Diagram illustrating the growth and negative charging of a hail pellet as it falls through a cloud of supercooled droplets. Droplets that hit hail pellets, freeze and eject small positively charged ice splinters which are carried towards the top of the cloud in the up-currents. Heavier hailstones carry a negative charge towards the cloud base.



CHARGING OF HAILSTONES

In order to investigate the relative importance of these two processes—the freezing and fragmentation of supercooled droplets, and the bouncing of ice crystals on the surface of hailstones—Latham and I have measured (in the laboratory) the rate at which artificial hailstones become charged by the impaction of small ice crystals and supercooled water droplets. The experiments were conducted in a cold room at temperatures down to −16°C. The simulated hailstone (a small sphere of ice) was held stationary in a wind tunnel as ice crystals (0·02 to 0·05 mm. diameter) or supercooled droplets (0·02 to 0·1 mm. diameter) were blown past it at speeds varying from 1 to 30 m./sec., covering the speeds at which most hailstones fall through clouds. It was possible to vary the temperature of the hailstone relative to that of the surrounding air. Temperatures were measured with thermocouples and the charge acquired by the hailstones was measured with a sensitive electrometer.

It was found, as might have been expected from the earlier work described above, that the sign of the electrical charge produced by collisions between ice crystals and hailstones was governed by the temperature difference between them; the hailstone became negatively charged when it was warmer than the rebounding crystals and vice versa. The magnitude of the charge was proportional to the temperature difference but rather insensitive to the size and impact velocity of the crystals. With a temperature difference of 5°C, a rebounding crystal having a diameter of about 0·05 mm. produced a charge of 5×10^{-9} e.s.u., on average. At temperatures within a degree or two of the melting-point, the crystals stuck on the surface of the hailstone and the charging ceased.

When supercooled droplets were used in this experiment, the growth of the hail pellet by the accretion and freezing of supercooled droplets was accompanied by the ejection of small splinters of ice, the hailstone acquiring a negative charge. Droplets less than 0·03 mm. in diameter produced a few splinters and little charge; when the size of the droplet was increased, the rates of splinter and charge production increased until (with large drops moving at higher speeds) splashing occurred on the surface of the hailstone and the charging was greatly reduced and even reversed. In a typical experiment at an air temperature of −15°C, droplets of 0·08 mm. diameter impacting at 10 m./sec. froze to produce, on average, twelve ice splinters and a charge of 4×10^{-6} e.s.u./drop. As before, these results were not appreciably affected by the presence of contaminants in the water in concentrations that are usually found in rainwater.

CHECKING THE THEORY

We are now in a position to use the results of these experiments to calculate the probable rates of charge generation which will be associated with the formation of soft hail in a typical thundercloud. Let us consider first the electrification produced when pellets of soft hail of radius R and mean density ρ fall at a velocity v through a cloud of much smaller ice crystals. The number of hailstone-crystal collisions per unit volume per second will be

$$\frac{dN}{dt}=E\pi R^2 n_c n_h v \qquad (1)$$

where n_c is the number of crystals per unit volume of cloud, n_h is the number of hailstones per unit volume, and E is the collision efficiency.

We may express this equation in terms of the equivalent precipitation rate p by writing

$$p=\frac{4}{3}\pi R^3 \rho n_h v \qquad (2)$$

If a crystal collision produces, on average, a charge q_c, the rate of charge production per unit volume of cloud becomes

$$\frac{dQ}{dt}=\frac{3}{4}E\frac{p}{R\rho}n_c q_c \qquad (3)$$

A reasonable value for the contribution of the soft hail to the precipitation intensity p in an intense storm would be 5 cm./hr. or 1/720 cm./sec., while $R=0{\cdot}2$ cm. and $\rho=0{\cdot}5$ gm./c.c. Aircraft measurements of ice crystal concentrations in large cumulonimbus clouds give typical concentrations of $n_c=10^5$/cu. m. or 0·1/c.c. for crystals larger than 0·08 mm.; for such crystals, we may put $E=1$. Finally, using the average value for q_c of 5×10^{-9} e.s.u. obtained in our experiments, we have

$$\frac{dQ}{dt}=5\times10^{-12}\text{ e.s.u./c.c.-sec.}$$
$$=10^{-4}\text{ coulombs/km.}^3\text{-min.}$$

We might arrive at a higher figure by assuming that the concentration of crystals smaller than 0·08 mm. diameter is considerably greater than the above figure but no realistic adjustment of the parameters would increase the computed rate of charging by the factor of 10,000 necessary to bring it up to our required value of 1 C/km.^{3}-min. We must conclude, therefore, that rebounding ice crystals can contribute only slightly to the mechanism of thunderstorm electrification.

Let us now consider the charge produced by the splintering of droplets as they freeze on hail pellets. The rate of charge production may be written in a manner very similar to equation (3):

$$\frac{dQ}{dt}=\frac{3}{4}E\frac{p}{R\rho}n_d q_d \qquad (4)$$

where n_d is now the concentration of supercooled droplets with diameters greater than 0·03 mm. (splinter and charge production being very small for smaller droplets) and q_d is the average charge produced per drop. Aircraft measurements in cumulonimbus clouds reveal that droplets larger than 0·05 mm. are present in concentrations of at least 1/c.c. Substituting $n_d=1$/c.c., $q_d=4\times10^{-6}$ e.s.u. (based on the above experiments) and the same values for the other parameters as in the previous calculation, we obtain

$$\frac{dQ}{dt}=4\times10^{-6}\text{ e.s.u./c.c.-sec.}$$
$$=1\ C/\text{km.}^3\text{-min.}$$

Since we have chosen what are probably minimal values for p and n_d, and since we need not restrict ourselves entirely to the small pellets of soft hail (larger hailstones will also contribute providing their surfaces remain dry), we conclude that this mechanism is indeed capable of generating and separating charge at the required rate in thunderstorms. We suggest that this is the principal mechanism.

If this is so, it would appear that the large-scale generation of electricity in thunderstorms and the origin of the lightning flash depend essentially on a thermoelectric property of ice—that is, the preferential migration of protons under the influence of a temperature gradient.

NEW WAYS OF CURING NEUROSES

H. J. EYSENCK

Studies made during the past ten years show that psychoanalysis is almost valueless. Conditioned-reflex therapy is proving to be more effective for treating neuroses than any method tried so far.

Neurotic disorders of one kind or another are believed to exist to a serious and incapacitating degree in between 5 and 10% of the population. Whether they have been increasing since the last century or not we cannot tell; methods of diagnosis, access to medical treatment, and the emergence of psychiatry as a recognised discipline have so transformed the picture that any comparison would be nothing but guesswork. Nevertheless the position clearly is serious, and neurosis is probably responsible for more human unhappiness and tragedy than any other single disease.

Neurotic disorders have always attracted a good deal of popular attention, not least because they appear to present a paradox. By and large, we all hold some kind of hedonistic philosophy—that is, we believe that people do the things they want to do, and do not do the things they don't want to do. Philosophically, of course, the position is very much more complicated than this, but, on the whole, human behaviour does not seem to diverge very much from this pattern. It is, however, one of the distinguishing marks of the neurotic disorders that they seem to contravene the logic of this position. Consider Mrs Jones, who is forty-two years of age, a mother of three children, and who is terrified of birds. Seeing a bird—even the most inoffensive little sparrow—produces in her a fear so excessive that she has severe palpitations, may even faint, and feels such a strong compulsion to run away that she cannot always control herself. This fear, which has been in existence for thirty years, has practically ruined her life. She dare not go out, in case she should see birds. The curtains have to remain drawn in case a bird alights on the window-sill. In fact, she has been reduced through her fear to living the life of a troglodyte. I need hardly spell out the suffering all this causes to her, to her husband, and to her children.

It is quite useless to argue with Mrs Jones and explain to her that her fears are groundless. She knows this perfectly well, and as far as logical, rational evaluation is concerned, she is no different from anyone else. She certainly does not want to have this fear, and would give anything to be rid of it; yet somehow she is incapable of achieving this. There is no malingering involved. A physiological record can be made of her heart-beat, pulse-rate, and blood-flow on being shown a bird, and all of these indicate the presence of an extremely strong emotion.

There are, of course, many other types of neuroses, most of which are characterised by the presence of strong fear and anxiety reactions. These may be tied to one particular stimulus, as in this case, or they may be much more widespread. Sometimes they interfere with behaviour, as in the case of stammering or writer's cramp, and sometimes they produce direct behavioural symptoms like tics (compulsive movements of some part of the body) or obsessional acts (like having to wash one's hands several hundred times during the course of the day). But whatever the particular symptom or set of symptoms may be, it is usual for the sufferer to be quite aware of its irrational nature, and to unquestionably want to get rid of it. This then is the nature of the paradox; we have a large number of perfectly rational, reasonable people, with intelligence quotients no whit lower than the general run of the population, but who do things they patently do not want to do and cannot do things they equally patently wish to do. What has gone wrong?

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FREUD AND PSYCHOANALYSIS

One explanation was put forward by Freud around the turn of the century, and has been widely accepted. Briefly, he posited that certain conflicts of a sexual nature arise in the mind of the young boy which make him wish to kill his father and marry his mother. Fear of the powerful father figure leads to the repression into the unconscious of this so-called Oedipus complex which, however, is not deprived of its energy and which may emerge in later life, leading to the peculiar symptoms experienced by the neurotic. Cure is possible only through an extensive psychoanalysis in which the analyst "uncovers" this potent source of all evil, thus draining it of its energy. Since the emergence of this theory, psychotherapy based in some way or other on psychoanalytic thinking has become the most widely used technique for the treatment of neuroses, at least in the United States and to a lesser extent in Europe. (Statistics indicate that this statement, as far as the United States is concerned, is true only of those earning more than $10,000 [about £3600]; for those earning less, electric shock is much the more common form of treatment.) Freud's hypothesis is important because it is based on a *deterministic* way of looking at the problem. Unfortunately, empirical studies conducted during the past ten years or so leave little doubt that, as a form of treatment, psychoanalysis is almost completely valueless. The evidence, which has been reviewed in great detail in the "Handbook of Abnormal Psychology" I recently edited, indicates that neurotic disorders tend to improve without any form of psychiatric treatment at all, that is by what is called *spontaneous remission*; of serious cases, about two out of three will have recovered or be very much improved after two years with purely routine GP treatment. The record of similar cases treated by psychoanalysis is not better, and may, indeed, be worse. The fact that there is no scientifically acceptable, positive evidence for the therapeutic value of psycholanalysis is now so widely accepted among experts that I shall not waste space in order to document this point any further; it is largely among laymen (teachers, social

workers, and others with only a vague smattering of psychology) that a belief in the efficacy of psychotherapy and the unique benefits of psychoanalysis is still found.

PAVLOV AND THE CONDITIONED REFLEX

If we must reject the Freudian theory and practice, what can we put in its place? The answer which is being accepted more and more widely derives from the work of the great physiologist and scholar I. P. Pavlov, and may be formulated in the following way: *neurotic symptoms are maladaptive conditioned responses.* Most people nowadays are familiar with the paradigm of the conditioned response: take a dog which salivates to meat (the unconditioned stimulus or UCS) but does not salivate to the sound of a bell, or some other neutral or "conditioned" stimulus (CS). If you now pair these two stimuli—by always ringing the bell just before giving the dog his food—after twenty or so repetitions, the dog will salivate to the bell even though no food may be forthcoming. The salivary reflex has become conditioned to the bell, and will occur whether or not its occurrence is useful to the animal.

Consider now the application of this to the experimental creation of a phobia accomplished by the well-known American psychologist Watson at the time of the First World War. He took an eleven-month-old boy Albert who was very fond of white rats; these constituted the conditioned stimulus. The unconditioned stimulus was provided in the form of a very loud, fear-provoking noise which Watson produced by banging an iron bar with a hammer whenever little Albert reached out for the white rat. A few repetitions were sufficient to condition the fear aroused by the noise to the white rat, and after this short programme of conditioning, little Albert showed all signs of fear and withdrawal whenever a white rat was introduced into the laboratory.

It is known from experiments with dogs and humans that there exists a so-called stimulus gradient which ensures that not only the conditioned stimulus but also other stimuli similar to it in some way will produce the conditioned response, although less strongly the less they are like the original conditioned stimulus. Thus, if a dog has been conditioned to salivate to the sound of a tuning-fork vibrating a thousand times a second, then the dog will also salivate to the sound of a tuning-fork vibrating at half or twice this rate although his salivation will be less copious. In the same way, little Albert now showed fear not only of white rats but also of rabbits and other furry animals, and even looked upon a teddy-bear with a jaundiced eye. There was thus set up in little Albert a perfectly typical and genuine phobia, the strength of which was considerable and could easily have been increased by either continuing the process or by using a stronger unconditioned stimulus, such as a really painful electric shock. Watson contended—and later work has borne him out—that this is indeed the paradigm for neurotic disorders. They are essentially fear reactions acquired through a straightforward process of conditioning.

THE CASE OF THE IMPOTENT HUSBAND

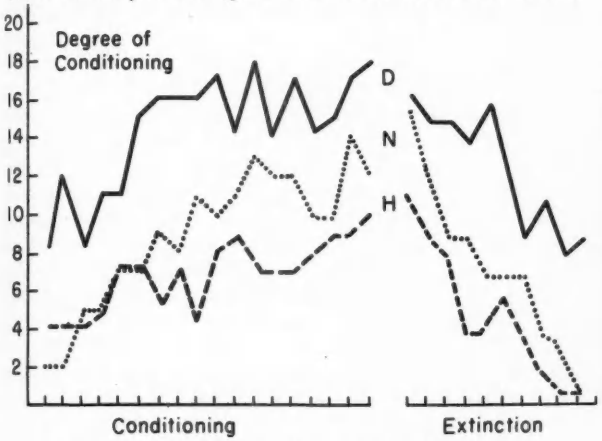
Consider now an example which will show how this kind of process may work in real life. The sufferer in this case was a man who complained of his continued sexual impotence with his wife whom he loved very much. Curiously enough, this difficulty only arose at home and not on holiday or abroad. Exploration revealed that this man, as a youngster, had seduced another man's wife, had been surprised in the act, and had been beaten within an inch of his life by the enraged husband. This treatment produced very strong fear responses which constituted, as it were, the response, while the beating constituted the unconditioned stimulus. What was the conditioned stimulus? In the laboratory we usually present a specific stimulus which catches the animal's attention and is specified beforehand; in a situation such as this, almost anything that happens to attract the patient's attention at the time the unconditioned stimulus is applied may serve as the conditioned stimulus. In the present case this happened to be the wallpaper which was directly before the patient's eyes when he received his punishment, and it will not surprise the reader to hear that by sheer accident the wallpaper in the patient's bedroom was identical with that which was so vividly impressed on his visual receptors upon that unfortunate occasion. The patient, of course, had forgotten all about the wallpaper and, indeed, the whole episode; his central nervous system, however, had not. Conditioned responses, once they are established, are very difficult to extinguish and will remain unimpaired in strength by the passage of time. Consequently, when the patient attempted to have intercourse with his wife many years later in a room, the wallpaper of which constituted the conditioned stimulus to a fear reaction, this fear reaction interfered with his potency through physiological processes that are well understood. The cure in this case was absurdly simple; what was required was not a four-year course of psychoanalysis but rather the services of a paperhanger!

A CLOSER LOOK AT THE PROCESS

It is important to realize just what it is that is being conditioned in cases of this kind. Mammals have two somewhat different types of nervous system. One is the central nervous system which mediates our sensory impressions (sight, touch, hearing, and so on) and our voluntary movements; the other is the autonomic system which is partly independent of the central nervous system and which governs a variety of activities which are mostly far removed from voluntary control. The beating of the heart, sweating, digestion, dilatation of the pupils, and many other automatic activities are governed by this system and it is noteworthy that most of them are closely linked with emotion. Consider a man who is angry or afraid. His heart pounds, his digestion is stopped to release more blood to the rest of the body, his eyes dilate to admit more light, sweat forms on his hands to make his grip on his adversary more secure, the saliva in his mouth dries up, etc.—there are a thousand and one physiological reactions the occurrence of which is due to the activity of the autonomic nervous system. Each is the accompaniment, or quite probably the cause, of what we introspectively identify as emotion. It is emotions of this kind which can most readily be conditioned, and it is these physiological indices of emotion which we usually record in our laboratory experiments.

It should be added that the autonomic system itself con-

sists of two divisions—the sympathetic and the parasympathetic—and that, by and large, these two systems are opposed to each other in their action. The accompaniments of fear and anger mentioned above are produced by the sympathetic system which is the energising, fight-flight system. All these effects are reversed by the parasympathetic system, the function of which is to let the animal vegetate quietly and peacefully. Thus, we might generalise and say that in many cases neurotic symptoms are *conditioned sympathetic reactions* that are linked with originally neutral stimuli which acquired the power to produce strong emotional reactions through the process of conditioning. A few difficulties may occur to the reader on this point. In the ordinary conditioning experiment, a number of pairings between CS and UCS are required, but is this likely to happen in ordinary life? A man may get beaten up once and cast his terror-stricken gaze upon the wallpaper, but is he likely to indulge in this habit a number of times,



Typical record of the acquisition and extinction of conditioned responses. This chart shows the acquisition and extinction of conditioned eye-blink reflexes for three groups: *D*, introverted neurotics (dysthymics); *N*, normals; *H*, extraverted neurotics (hysterics). As is usual, the introverted subjects became conditioned more rapidly.

and are all the women he seduces likely to have the same wallpaper in their bedrooms? Clearly we must posit, rather, some kind of traumatic event in which the unconditioned stimulus produces such a strong reaction that conditioning occurs in one trial. This does not raise any real difficulties as there is experimental evidence from the laboratory that one-trial conditioning does actually take place. In many cases, of course, it is quite likely that neurotic disorders are produced by an accumulation of conditioning experiences. This is so when the patient finds himself in a situation where he is, for example, constantly humiliated by his employer or by a superior officer; anything pertaining to the person in question may constitute the conditioned stimulus, and the emotional reaction to the humiliating situation constitutes the conditioned response. It will be necessary in each case to discover in what way the conditioning paradigm can in fact be applied to the neurotic symptom in question.

Another difficulty that arises is this. Conditioned responses tend to extinguish when the conditioned stimulus is not accompanied by the unconditioned stimulus. You have conditioned your dog to salivate to the sound of a bell, but if you now ring the bell a number of times without giving him food he will soon cease to salivate. The conditioned response is obviously an extremely useful device of nature for signalling future events to the animal, but in order to remain useful it must also be flexible and cease to act as a signal when there is no longer any connexion between it and the signal-led event. If this is so, and if a conditioned fear-response has been produced by a single pairing of the conditioned and the unconditioned stimuli, then why is it that we do not get a rapid extinction on many occasions when the conditioned stimulus is not followed by the unconditioned stimulus?

There are two answers to this question. In the first place, it seems likely that the spontaneous remission so characteristic of neurotic disorders is due precisely to this mechanism. The failure of the conditioned stimulus to be followed by the unconditioned stimulus gradually leads to

Arrangement for the eye-blink conditioning experiment, the subject fixating a small red light on the wall. To condition the subject, a tone is delivered over the earphones at the same instant that a puff of air from the compressed air cylinder is blown through a tube towards his or her eye. After this is repeated a number of times, the subject begins to blink when only the tone is heard.

an extinction of the symptom. However, this is not always so, and there are many cases on record—the bird woman mentioned above, for example—where a phobia has lasted twenty or thirty years, or even longer. In these cases, what seems to have happened is the following. The process of extinction in the laboratory is relatively quick because (*a*) we can present the conditioned stimulus without the unconditioned stimulus whenever we want to, and (*b*) we can make sure the animal is there to attend to the stimuli by tying him to his stand. With people, however, we cannot do this in ordinary life and what happens may be rather like this. A person is afraid of a certain class of objects through a process of single-trial conditioning. An object of this type approaches, or is being approached by the person in question. The conditioned reflex immediately starts to work and he begins to feel fear and anxiety. These feelings make him turn on his heels and escape from the object in question and this escape is accompanied and followed by a reduction in the conditioned fear he has felt. He is thus rewarded (and reinforced) for running away from the

object and a habit of running away from the object each time is set up. There is consequently no possibility for extinction to occur; on the contrary, he develops a very strong habit of avoiding the object and making what is sometimes called "reality testing" impossible. We can easily explain in this way the long-term continuance of some neurotic symptoms. The bird woman was not only afraid of birds but she made it impossible for this phobia to be extinguished by making certain that she would never encounter a bird at any time; this is typical of many refractory cases of neurotic disorder.

THE USE OF CONDITIONING AS A CURE

So much about the theory of causation; how about therapy? Modern learning theory has made a special study of the growth and extinction of habits. (The conditioned response is officially classed as a "habit" in psychological terminology; a usage which may not commend itself to common sense but which has many advantages and will be followed here.) One of the most potent and, indeed, most obvious ways of breaking a habit is the creation of a counter-habit which is incompatible with the original one. In the case of the neurotic disorders we have been dealing with, we have diagnosed the trouble as being due to a conditioned sympathetic reaction which has ceased to be, or never was, adaptive; to get rid of this sympathetic reaction, what we need to do is clearly to condition a parasympathetic reaction to the same stimulus or set of stimuli. As explained before, the actions of the sympathetic and parasympathetic system are antithetical; what the one does the other one undoes. The use of this method of "reciprocal inhibition", as it has been called by Prof. J. Wolpe, who is its main proponent, may be illustrated by going back to the case of little Albert. We left him afraid of furry animals and reluctant to be near a rat or a rabbit; how can we condition the appearance of these animals to a parasympathetic response? The sympathetic system stops digestion while the parasympathetic stimulates it; perhaps offering the hungry infant some chocolate while in the presence of the animals might produce the desired effect. This is difficult, however, as the sympathetic reaction of the subject is so violent as to override the parasympathetic reaction to chocolate. Suppose we take Albert to a large room and introduce a white rat to him as far away in this room as possible. He will cast some agitated glances at it but will not actually panic as long as it does not run towards him. In this situation he is willing to take the chocolate, thus producing an increment of parasympathetic conditioning to the rat which offsets a portion of the conditioned sympathetic reaction. The next time, therefore, the rat can be brought nearer and the same process repeated, until finally after ten or so trials the infant sits happily munching his chocolate while stroking the rat. In the case of adult neurotics, of course, all this is much more difficult, as they are usually less impressed with chocolates, and also because the physical distance between the object and the person is often less important than the psychological distance. Nevertheless, the procedure can be adapted without too great difficulties. We can produce parasympathetic reactions in various ways—by training and relaxation, by hypnosis, by drug treatment, possibly even by sympathetic listening, and so on. The psychological distance can be created by dealing not with the object itself, but by using pictures of the object, or just the name of the object, talking about the object, and so on. Through generalisation, the parasympathetic effect will then spread from these substitute objects to the real one, reducing the amount of conditioned sympathetic reaction to the stimulus appropriately.

Let us apply this method to the bird woman. We might start by talking to her about birds in a quiet relaxed discussion; after a while we might introduce small pictures of parts of birds at some distance, gradually working up to pictures of real birds brought nearer and nearer. We might go on from there to films of birds, stuffed birds, and so forth, finally ending up, as in the case of little Albert, with tiny chickens, a few days old, introduced far away from the patient at the other end of the room. This is a very oversimplified description of what occurs, of course, but it will suffice to give the reader an idea of the method used in these cases. A much more detailed description, together with detailed case histories, will be found in Wolpe's book, "Psychotherapy by Reciprocal Inhibition".

I have purposely given as an illustration an extremely simple monosympathetic phobia which is not really typical of most neurotic disorders. Usually we have a whole range of stimuli which produce anxiety; these may all be of a particular kind (animals, for instance) or they may be quite diversified. In these cases it is necessary to take each group of fear-producing stimuli in turn and destroy the effectiveness of the sympathetic response by reciprocal inhibition. This is much more complex, but not different in principle from the kind of case already discussed.

How about the effectiveness of this method of treatment? From his treatment of a large series of unselected neurotics Wolpe finds that this method, compared with psychoanalytic treatment, is (*a*) very much shorter, and (*b*) very much more effective. Statistical comparisons of this kind present many difficulties and must be evaluated very carefully before they can be accepted at face value but Wolpe's original results and those of his many students and followers leave little doubt in my own mind that we have here a rational method of treatment that is more effective than anything that has ever been offered for the treatment of neuroses. It will be many years, however, before the precise limitations of the method are fully understood and before its application to the many different types of neurotic disorder have been worked out in sufficient detail to make its routine use possible by psychiatrists not trained in psychological methods, learning theory, and conditioning.

INTROVERTS, EXTRAVERTS, PSYCHOPATHS, AND HYSTERICS

It is interesting to link up this method of treatment and the theory of the genesis of neurotic symptoms with certain personality differences in conditioning. Pavlov found quite early that dogs differ very much in the speed with which they formed conditioned responses, and the strength of these responses. Human beings also differ very much in the speed of formation and the strength of conditioned responses. A great deal of evidence is available to show that it is introverted people (those who are socially reserved and

introspective, who value thought more than action, and who prefer reading and thinking to the world of social life) who are particularly easy to condition. As conditionability is almost certainly an inherited property of the nervous system, this suggests that introverted people are genetically predisposed to develop phobias and anxieties of a neurotic nature. This has indeed been shown to be so. People who are suffering from anxieties and fears of a neurotic type tend, in fact, to be introverted and to be exceptionally easy to condition.

This should not lead us to believe that introversion is much the same thing as neuroticism. There are extraverted types of neurotic symptoms which are very different from those we have been discussing so far, and which are linked with the lack of conditionability of the extraverted person.

Conditioned responses are not always maladaptive; indeed, in the majority of cases they are probably not only extremely useful but essential to survival. If the bird woman had acquired a fear of snakes, rather than of birds, this would have been adaptive, and it might even have been instrumental in saving her life. Similarly, many of our socialised reactions (our responses to moral codes and to the mores of our society) are probably due to conditioned autonomic responses acquired in early childhood, which produce fear and anxiety whenever we are tempted to make overt sexual or aggressive reactions in situations which we have been conditioned to regard as unsuitable. Looked at in this way, we may say that *conscience is a conditioned response*, and it is clear that people who are defective as far as their conditioning equipment is concerned would be liable to be *under-socialised* and lacking in this quality of "conscience".

Now there does exist a group of neurotics, the so-called *psychopaths* (and to a lesser extent the hysterics), who are characterised precisely by this lack of conscience or social sense. Indeed they are sometimes called *moral imbeciles* to draw attention to their failure to behave in a morally and socially acceptable manner in spite of their intelligence. Pathological liars and swindlers belong to this group, as well as unregenerated rakes, confidence tricksters, and so forth. On a lower level you get the soldier who constantly goes AWOL despite the certainty of being found out and punished, or the boy who constantly plays truant, or the girl who from an early age indulges in sexual offences. We would expect that psychopaths and hysterics would prove difficult to condition in a laboratory experiment and this is indeed so. There are several experiments showing that where anxiety neurotics may be conditioned after two or three pairings of the conditioned and unconditioned stimulus and where normal people might require twenty or so pairings, psychopaths and hysterics might only condition after between thirty and a hundred pairings, and sometimes not even then. Little has been done in the way of treating hysterics and psychopaths by means of behaviour therapy—the name given to methods of treatment deriving from modern learning theory and the conditioning paradigm. This is perhaps due to the great difficulty of instilling the necessary conditioned response in adults who form these responses only with the greatest difficulty.

There is, however, one field in which a good deal of experimental work has been done on the creation of a conditioned response, the lack of which was responsible for much anxiety and unhappiness. I am referring to *enuresis nocturna*, or simple bed-wetting—a complaint characteristic of perhaps the majority of children sent to child guidance centres. In the normal child, extension of the bladder produces waking; in the enuretic child the extension of the bladder leads to urination before waking takes place. What the psychologist does is to make a child sleep on a quilted blanket, woven into which are two electrodes leading to a battery and a loud alarm bell. When the child begins to urinate, the circuit is closed and the bell rings, waking him up less than a second or so after he has started to urinate. The extension of the bladder which precedes urination is the conditioned stimulus, the bell is the unconditioned stimulus, and waking up is the response. By pairing the extension of the bladder with the bell a number of times, the former acquires the waking powers of the latter through simple conditioning; after between three and thirty repetitions most children lose their neurotic habits and become normal. It has been found that this method is markedly superior to psychotherapeutic efforts in getting rid of this particular troublesome symptom.

AN ARGUMENT AGAINST THE FREUDIANS

Freudian psychiatrists sometimes object that behaviour therapy only gets rid of the symptom but not the underlying complex, and consequently that the removal of the symptom will only be followed by a resuscitation of the symptom, or the emergence of another one. This is indeed what should happen if Freud's theory were correct; the fact that it does not happen is an added proof of the incorrectness of the Freudian formulation. Several detailed follow-up studies of the effects of behaviour therapy have utterly failed to disclose any such return of the symptom, or re-emergence of another one. The evidence on this point is particularly valuable as this is one of the few cases where psychoanalysts have made a definite prediction which can be tested experimentally. As this point is central to psychoanalytic theory, its disconfirmation must be regarded as particularly important in any evaluation of the Freudian system.

It is, of course, impossible in the confines of a short article like this to discuss the many points which arise for discussion, or to describe the many methods currently being used in this field. The reader whose appetite has been whetted but not satisfied by this brief account can be referred to the book by Wolpe already mentioned or to my own "Behaviour Therapy and the Neuroses" which discusses all these problems in considerable detail. I do not wish to end, however, without stating explicitly that while I think the theory discussed here has considerable merit and is almost certainly on the right lines, it is at present no more than a theory in the process of being evaluated. It should not be regarded as a law of nature. There is little doubt that the messianistic way in which psychoanalysts have propagandised in favour of their own particular set of theories has retarded the scientific progress in this field very seriously; it is to be hoped that the contentions of behaviour therapy will receive a more serious and critical welcome.

DETECTING FAINT GALACTIC CLOUDS

GEORGES COURTÈS

The development of new observing methods at the Haute Provence Observatory has now made it possible to detect weakly emitting galactic clouds that were formerly unobservable because of the strong absorption of light in the galaxy and the lack of contrast between monochromatic and confinuum light. The contrast between faint monochromatic light sources and the background light has been increased by a factor of 75, making it possible to detect clouds (emitting hydrogen-alpha or other radiations) whose brilliance is seventy-five times less intense than those of clouds that can be photographed with a telescope that is equipped only with a colour-filter. This has already led to the detection of an ionised region in the core of the very large neutral hydrogen cloud which surrounds the galactic centre; the techniques used have also made it possible to measure the relative velocity of this cloud in order to identify it with the expanding and rotating disc found by radio telescopes.

Before discussing the techniques and results, however, some of the background will be reviewed.

THE OPTICAL DETECTION OF THE CENTRAL REGIONS OF THE GALAXY

The centre of rotation of our galaxy —long known from the study of the general motion of the stars and the determination of the centre of symmetry of the globular clusters—is situated in the direction of the constellation of Sagittarius, 8500 parsecs from the Sun (1 parsec=3·26 light-years or $1{\cdot}92\times10^{13}$ miles). Up till now, photographic methods—the only ones which give an exact image—have been unable to detect the central regions of the galaxy because these regions are hidden from view by very dense, absorbing clouds. Radio waves have been detected coming from the direction of the galactic centre, however, because these waves are not absorbed by the dust clouds between us and the centre. The strongest signals picked up by radio-telescopes have come from Sagittarius A, which has been identified as the very centre of the galactic system; its radio spectrum is continuous.

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21-CM. EMISSION

The 21-cm. radio waves emitted by neutral hydrogen clouds—the only monochromatic radio emission found so far—is produced by the transition between two levels of the hyperfine structure of the fundamental states of neutral hydrogen. Because of its monochromatic character, it is possible to measure the velocity of a hydrogen cloud by a simple determination of the change in wavelength caused by the Doppler-Fizeau effect, as given by the equation

$$\frac{\Delta\lambda}{\lambda}=\frac{v}{c}$$

where $\Delta\lambda$=the Doppler-Fizeau change in wavelength

λ=the wavelength of radiation from the source when stationary

v=the radial velocity of the source

c=the velocity of light.

In measuring the radial velocity of the neutral hydrogen with the 75-ft. radio-telescope at Dwingeloo, The Netherlands, J. H. Oort and G. W. Rougoor discovered an extraordinary and unexpected phenomenon—the expansion of the neutral hydrogen in the central regions of the galaxy. When one looks in the direction of the centre, the circular motion of the clouds round this centre should give no indication of radial velocity; however, one finds a Doppler-Fizeau effect corresponding to a radial velocity of +200 km./sec. (receding) for the emitting regions and −150 km./sec. (approaching) for the absorption regions which one observes in the strong continuous background of Sagittarius A. The latter effect is apparently being produced by clouds of neutral hydrogen between the galactic centre and the Sun that are approaching at a speed of up to 150 km./sec.; the strongest component corresponds to an approach of 53 km./sec. The receding effect is obviously due to the outward motion of the clouds beyond the centre.

A detailed analysis of the neighbouring regions has enabled Oort and Rougoor to give the following interpretation:

Starting from the centre, there is a very flat disc 100 parsecs in diameter with a very rapid rotation that has not been calculated up till now. The density of hydrogen in the very centre is difficult to calculate but it is certainly very high, since it is already 100 atoms/cu. cm. at a distance of 20 parsecs from the centre. At 50 parsecs (the edge of the flat disc) the hydrogen density is 3 atoms/cu. cm. or more. It decreases as one goes out to a distance of 300 parsecs and falls practically to zero as one approaches 500 parsecs. At this point there is a sharp increase, indicating the presence of a ring 100 parsecs thick. This ring—it is perhaps a spiral arm—moves at a rotational speed of 265 km./sec., its angular velocity being twenty times greater than that of the Sun round the centre of the galaxy. The hydrogen density in this ring is in the order of 1 atom/cu. cm.

Farther away from the centre, at about 3000 parsecs, one finds a spiral arm which causes the absorption of the 21-cm. radiation (Doppler-shifted by the velocity of expansion of 53 km./sec.). Beyond this, the movement again become circular.

OPTICAL DETECTION

When a star rich in ultra-violet radiation (for example, hot stars O and B) is found in the neutral hydrogen, it ionises this hydrogen out to a considerable distance, the distance (for a given hydrogen density) depending on the

FIG. 1 (*page opposite*). The large cloud of stars of Sagittarius, photographed with a camera having a large field (25 degrees), and using a filter with a pass band of 120 Å. The central parts of Sagittarius are hidden by a diverse absorbing cloud. The centre of the galaxy is indicated by the black dot.

M 8
E15
E16
Centre
E12
E13
131
122

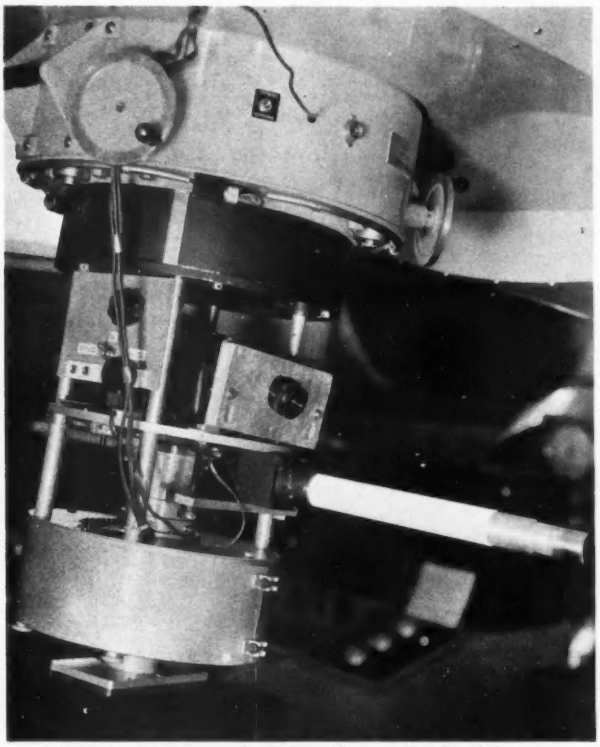
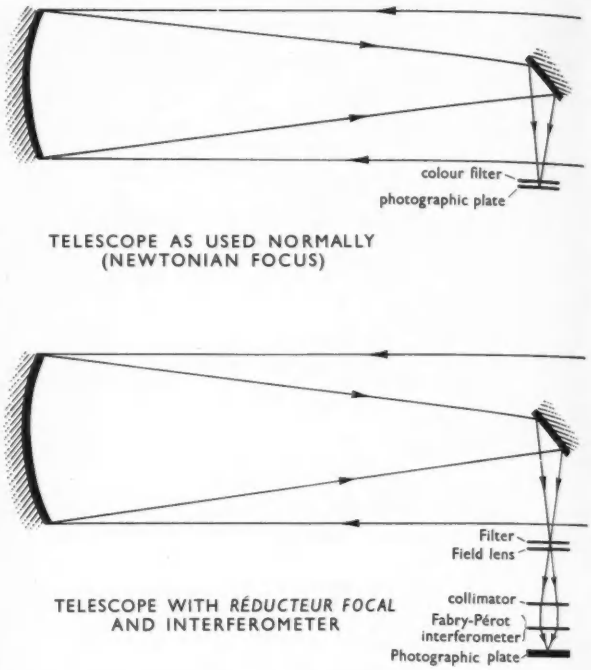


FIG. 2. A. A photograph of the *réducteur focal* and the thermostat of the Fabry-Pérot interferometer on the 75-in. telescope at the Haute Provence Observatory.



B. Diagrams showing the telescope as used normally with a colour-filter (*top*) and the arrangement using the *réducteur focal* and interferometer (*below*).

spectral type of the exciting star; the recombination of protons and electrons produces radiation of the hydrogen spectrum (in the visible region it gives the Balmer series with Hα, Hβ, Hγ, Hδ, and so forth). Of these radiations, the one that is least absorbed in passing through clouds of interstellar dust is the one with the longest wavelength—the Hα line having a wavelength of 6563 Å; it is also the most intense.

For these reasons, the Hα line has been used in intensive studies of the regions of optical emission, particularly in the very important Palomar Sky Survey. The 48-in. Schmidt telescope used in this survey did not reveal anything in particular near the galactic centre except four weak Hα regions. However, as we shall see, these are not associated with the clouds of hydrogen detected at the centre by radio. The latter are shown in Fig. 1, marked E12, E13, E15, and E16, according to the catalogue of S. Sharpless.

We now can detect these weak monochromatic hydrogen-alpha clouds in the following way. The method ordinarily employed involves the use of a simple colour-filter whose bandpass, together with the spectral characteristic of the photographic emulsion, produces a pass-band as narrow as possible in the spectrum of the background light. The intensity peak of this pass-band is adjusted as well as possible on the Hα line.

In this way one can obtain pass-bands that are 250 Å wide. Without this precaution one would obtain all the radiation in the 6700 to 3500 Å band to which the photographic plate is sensitive, including light from all the stars, the zodiacal light, and monochromatic emissions from the ionosphere; the exposure time would need to be too short to detect weak nebulae. With a filter, however, the light from the background of the sky is reduced, making it possible to use an exposure time of several hours—enough to collect the weak monochromatic light of the nebulae on an emulsion.

But there are other problems. The difficulties of manipulating a large telescope and the variations in weather and atmospheric turbulence hardly allow an exposure time of more than four or five hours. As the hydrogen-alpha clouds are extended sources of light, it was considered desirable to try to increase the focal ratio of the telescope as well as the exposure time. This led me to design an apparatus at the Haute Provence Observatory a decade ago which I called a *réducteur focal* (Fig. 2); it transformed the F/5 focal ratio of the telescope (Newtonian focus) into much larger focal ratios—F/2 and F/1. With an F/1 ratio, the light reaching the photographic emulsion was twenty-five times larger than it was with the original focus. Similar devices were independently developed later by J. Ring in England and A. B. Meinel in the U.S.A., giving them some interesting results.

To reap the full benefit from this improvement, one must, of course, use a very selective filter to eliminate as much of the stray radiation as possible and permit a long exposure time without the background of the sky over-exposing the emulsion. These conditions

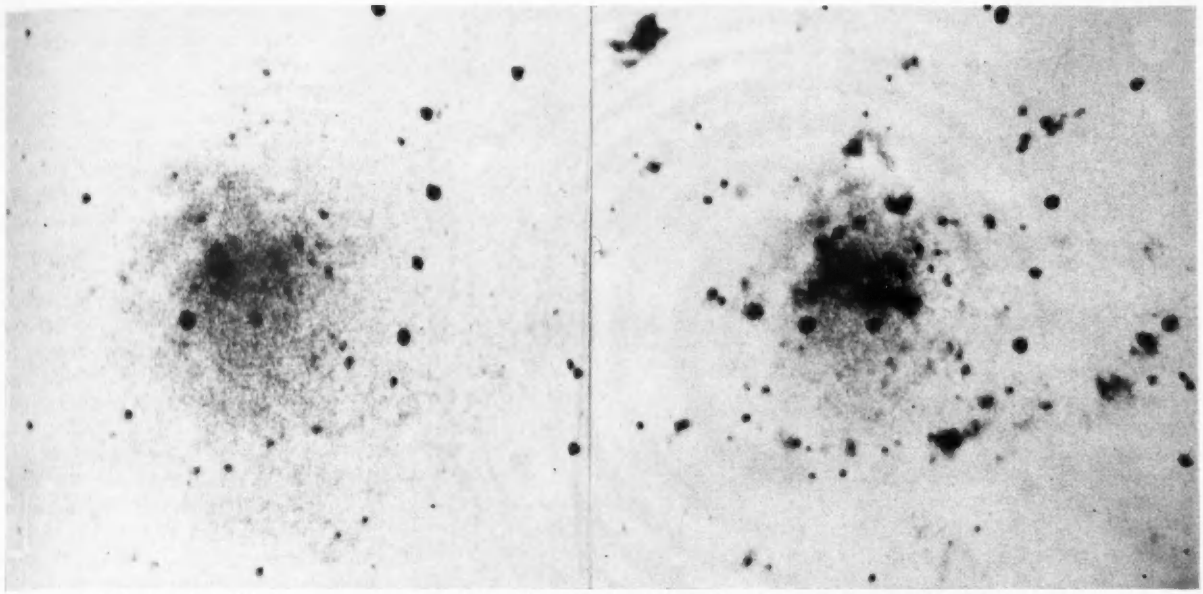


FIG. 3. Photographs of the extra-galactic nebula M33, taken with the *réducteur focal* mounted on the 75-in. telescope. At the right, one can see the intense image of the emission clouds transmitted by a Hα filter. At the left is the relatively weak image of the continuous background of stars, transmitted by a filter whose pass-band is outside the Hα line, at about 6400 Å. (The Hα line is at 6563 Å.)

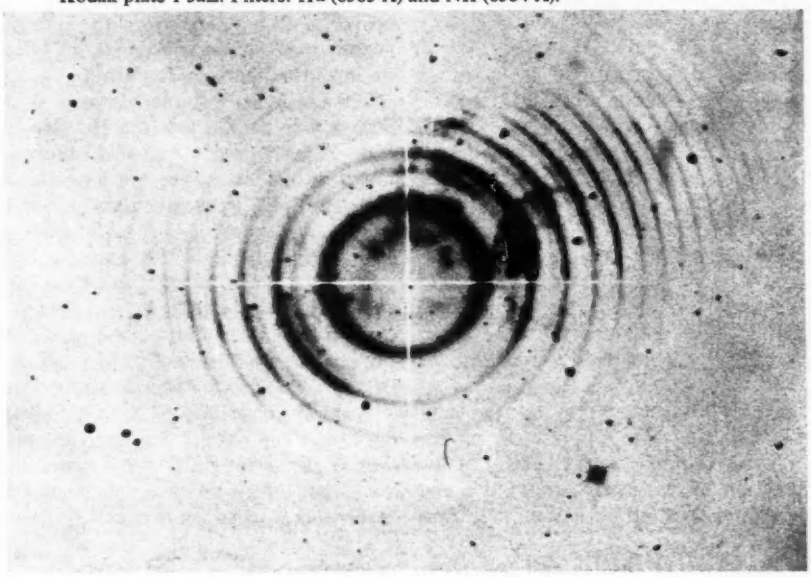
can be fulfilled with interference filters which have pass-bands of 50 Å and transmission efficiencies that can exceed 80%; Ring has shown that these conditions can be met with even narrower pass-bands.

Using filters with pass-bands of 50 Å, the contrast of monochromatic clouds against the background of the sky is about five times greater than that obtained with coloured filters having pass-bands of 250 Å. An example of the results obtained is shown in Fig. 3; these are photographs of the extra-galactic nebula M33 which have been taken with the *réducteur focal* mounted on the 75-in. telescope at the Haute Provence Observatory. One can see here the intense image of the emission clouds transmitted by a Hα filter (at the right) and the weak image of the continuous background of the clouds of stars transmitted by a filter whose pass-band is outside the Hα line, at about 6400 Å (at the left).

Previous to this, only the most intense of these nebulae had been detected (on the spectrograph by N. U. Mayall at the Lick Observatory, U.S.A.); as a result of the reduction of light from the very large continuous background of the central region, our equipment has revealed for the first time the large clouds of hydrogen near the centre. Several galaxies are now being observed in this way in Haute Provence.

Although this technique is still inadequate for observations of the central regions of the galaxy because of the strong obsorbing clouds, one can increase the gains obtained with the *réducteur focal* by using an interferometer. The insertion of a Fabry-Pérot interferometer (with multiple dielectric layers) in the parallel beams of the *réducteur focal* allows one to superimpose an interference pattern of rings on the image of the nebulae (Fig. 4). At the intensity peak of the rings, the

FIG. 4. NGC7635 (distance 2800 parsecs) taken with the 75-in. telescope using the *réducteur focal* and a Fabry-Pérot interferometer. F/1, 1·5-hr. exposure, Kodak plate 1·3aE. Filters: Hα (6563 Å) and NII (6584 Å).



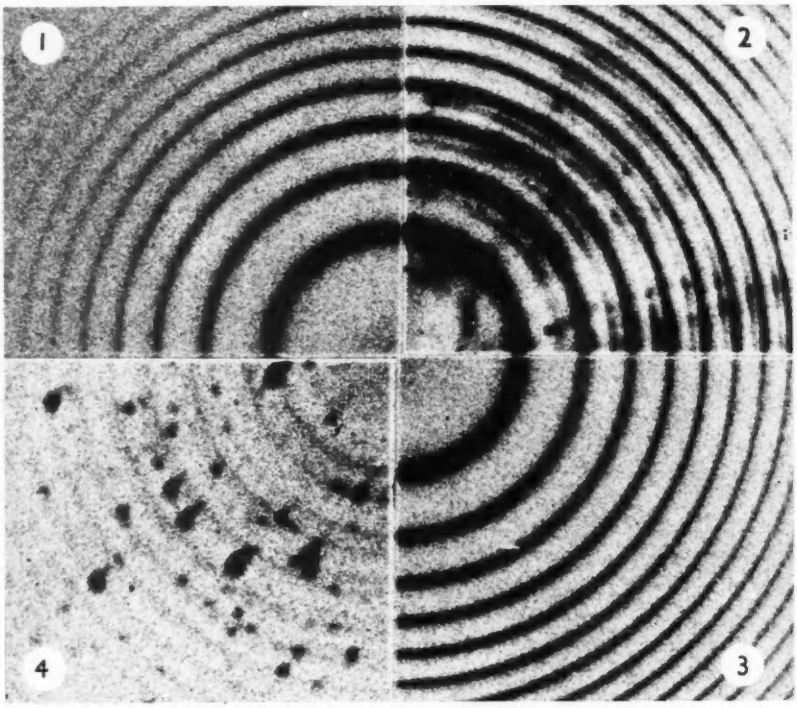


FIG. 5. Photographic montage of interference rings from (1 and 3) a calibration tube of hydrogen, (2) the nebulae M8, and (4) the region in the direction of the central bulb of the galaxy. Although this plate was taken with the same optical system as that of Fig. 4, the quality of the stellar images has deteriorated because of the strong atmospheric turbulence near the horizon. F/0·95, Kodak plate 1·3aE. Exposure: 10 min. for M8, 3 hr. for galactic centre.

light from the nebulae is almost entirely transmitted; between the bright rings the transmission is scarcely more than 5%. With this addition, the contrast relative to the sky can be increased by a factor of about fifteen.

The total gain, therefore, is an increase in contrast by a factor of seventy-five, making it possible to detect monochromatic clouds (emitting Hα or other radiations) whose brilliance is 1/75th as intense as those that can be photographed with a telescope using a colour-filter alone.

This apparatus permitted the detection of numerous emission clouds, but the very low height of the galactic centre above the horizon in the south of France has made it impossible to systematically explore the regions of Sagittarius. We have only been able to establish that the four clouds of Hα emission situated almost exactly in the direction of the radio centre are not directly associated with it as has been thought.

The main proof has resulted from the use of the Fabry-Pérot stage—it has enabled us to determine the radial velocity of the Hα clouds by simply measuring the radius of the rings of the interference pattern. These radial velocities do not correspond to the high negative velocities observed by the radio-astronomers at Leyden (−50 to −200 km./sec.) but are between +13 and +31 km./sec. After this observation, it was certain that the Hα clouds found at Palomar were not associated with the radio hydrogen cloud around the galactic centre.

It was only in 1959 that further gains were made as a result of (*a*) the excellent critical study of the absorption in the direction of the galactic centre by the American astronomer Hugh Johnson who was at the Commonwealth Observatory at Canberra, Australia, and (*b*) the very kind invitation of Dr Stoy, HM Astronomer at the Cape, which enabled me to make observations from the Royal Observatory at Capetown.

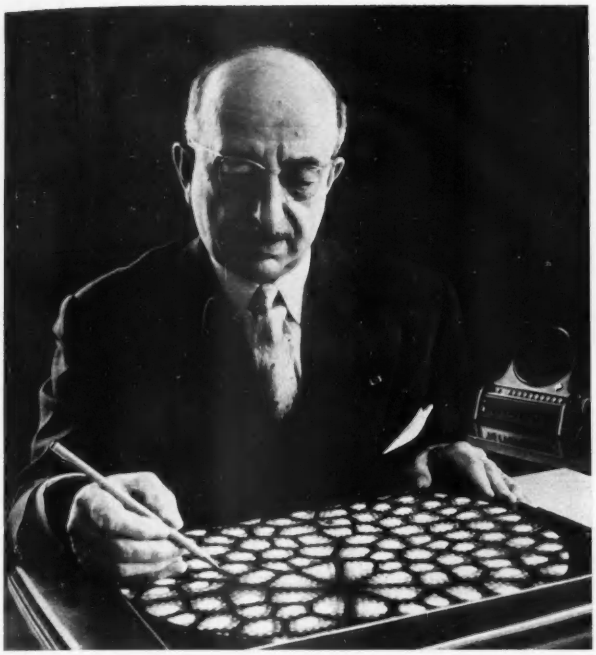
The plates taken at the Cape showed several weak fragments of interference rings. The measurements, though difficult, revealed an extremely strong Doppler-Fizeau effect of the same order of magnitude as that found for the 21-cm. radio emission. Unfortunately, the resolving power of the 6-in. telescope taken to the Cape was not adequate and a more detailed study was necessary.

Further observations were made at Haute Provence with P. Cruvellier, using the 75-in. telescope. In spite of the difficulties involved in observing the galactic centre at this latitude, we were able to obtain three-hour exposures that were easily measurable.

The results confirmed those obtained at the Cape. One can see the weak monochromatic message in the lower left-hand sector of Fig. 5. To make a comparison, we also took a plate of the nebula Messier 8 (upper right-hand sector—ten-minute exposure) which is situated in the spiral arm of the galaxy relatively close to the Sun (see Fig. 1); the coincidence of the Hα rings from Messier 8 with those of the calibration tube of hydrogen (upper-left and lower-right) is perfect, the Doppler-Fizeau effect from Messier 8 being very weak.

The two sets of rings in the lower left-hand sector of Fig. 5 were obtained from the direction of the galactic centre. The Hα set corresponds to a velocity of −188 km./sec. (approaching); the other is due to the 6584 Å nitrogen radiation, which appears in a great number of emission nebulae. The rings are apparently from a cloud that is very probably an ionised region in the core of the large neutral hydrogen cloud which surrounds the galactic centre. This photograph is one of the deepest probes of our galactic system by optical means. The most intense region of this cloud is situated 1° from the radio centre, the centre itself giving only a very feeble system of rings that overlap the most intense rings of the Hα radiation emitted by the regions common to the four clouds already mentioned.

The exact extent of the cloud is difficult to estimate at present, particularly because of the superposition with the nearer clouds. Numerous observations will still be necessary with interferential techniques to select the various components of the Doppler-Fizeau effect in this very complex region.



(*Photo Yan*)
Prof. Gaston Dupouy, Director of the Electron-Optics Laboratory at Toulouse.

OBSERVING LIVING SPECIMENS WITH THE ELECTRON MICROSCOPE

G. DUPOUY
F. PERRIER
L. DURRIEU

Although the resolving power of the electron microscopes is now so high that we can see details less than 10 Å in size at magnifications in the order of 1,000,000×, the instrument has grave limitations that further restrict its range of application. These are, in particular, (*a*) the need to introduce objects into a vacuum and (*b*) the fact that the penetrating power of the electrons is in most cases too low for many observations. Let us consider this more closely.

1. The electrons must travel through as high a vacuum as possible in order not to be subject to absorption and scattering from collisions with gaseous molecules. When living matter (bacteria, cells, etc.) is introduced into the vacuum area of the instrument, it dries out and the cell walls burst under the pressure. *Only dead objects can be observed.*

2. The low penetrating power of existing electron microscopes severely restrict their use; bacteria of a thickness as low as 1 μ are practically non-transparent to kV electrons, for instance.

The authors are associated with the Electron-Optics Laboratory (Toulouse) which is part of France's National Centre of Scientific Research (CNRS). Prof. Dupouy is the director of the laboratory and also the Honorary Director-General of CNRS.

Histological examinations with these microscopes can only be carried out properly by obtaining sections no more than a few hundred angstroms thick. ($1\mu = 10{,}000$ Å $= 0{\cdot}001$ mm.)

For these reasons, it has so far been impossible to photograph living matter with the electron microscope.

This article aims to show that we have solved this difficult problem using an electron microscope at exceptionally high voltages. Our microscope is designed for voltages up to 1500 kV. For the time being we are working mostly at 750 kV, although we have already been able to operate a little above 1 million volts.

It may be well to recall that at such accelerating potentials the electrons become highly relativistic, as their velocity may reach a significant fraction of that of light. In these circumstances, the potential difference V used to accelerate the electrons must be replaced by the effective potential difference V*, which is significantly higher. V* is given by the formula

$$V^* = [1 + 0{\cdot}9785 \times 10^{-6} V]\ V$$

For different values of the accelerating potential V, the following table gives the corresponding values of V*, the wavelength λ of the wave associated with the electrons, and $\beta = v/c$ where v and c represent the velocity of the electrons and of light respectively.

V (kV)	50	500	750	1000	1500
V* (kV)	52·45	744·6	1300	1978	3702
λ (Å)	0·0536	0·0142	0·01075	0·00872	0·00637
$\beta = v/c$	0·413	0·863	0·914	0·942	0·967

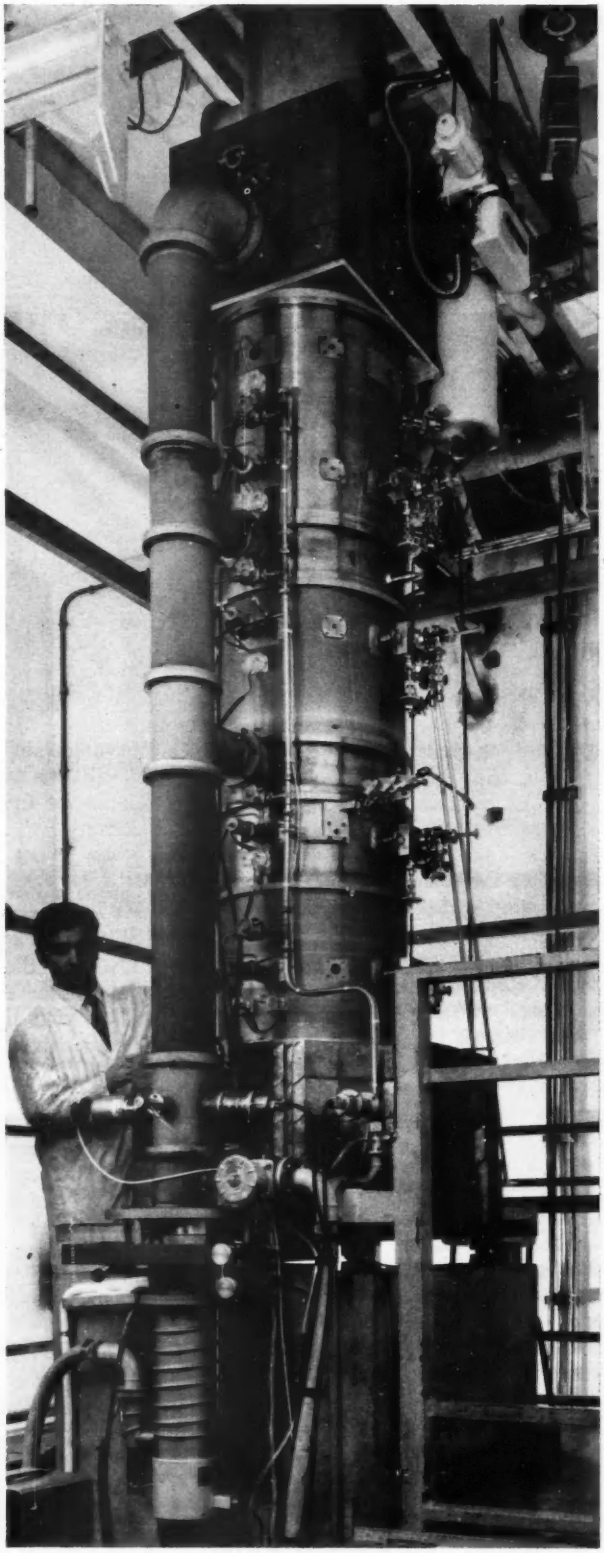
It will be seen, in particular, that at an operating voltage of 750 kV the relativistic effective potential difference is 1300 kV, while at 1 million volts it is very nearly 2000 kV. The relativity effect is very strong.

With electrons of this energy, membranes of collodion, carbon, etc. that are 600 Å thick are highly transparent and objects 1 μ in thickness become sufficiently so to be directly observable by transmission.

The object-cell. The preceding considerations led us to substitute a small sealed container or *object-cell* for the customary slide.

This unit is designed to keep the specimen to be examined in air at normal atmospheric pressure under appropriate conditions of humidity, that is, *in an environment in which cells may remain alive.*

Two cylindrical apertures O_1 and O_2 of very small diameter form a narrow



(*Above*) Part of the 1·5 million volt generator.

(*Left*) A general view of the electron microscope.

(*Below*) The Electron-Optics Institute at Toulouse. Sphere houses high-voltage generator.

(*Photos Yan*)

slit through which the electrons pass. The plane parallel faces at F_1 and F_2 are covered with a very thin film of parlodion-carbon, 600 Å thick; on drying, these layers take on the appearance of drumskins drawn taut over the apertures O_1 and O_2 and thus constitute two windows that are transparent to electrons. The thickness of the layer defined by the distance between the two windows F_1 and F_2 can be adjusted by regulating the pressure on the resilient gaskets R_1 and R_2; we varied it from approximately 2 to 10 μ. It is important that the path of the electrons through the air within the object-cell should be kept as short as possible in order to reduce scattering and absorption.

Finally, we provide a permanent connexion from the interior of the object-cell to an atmosphere of humid air by means of a metal tube T of miniscule cross-section with an internal diameter not exceeding 0·15 mm.

An exploded view of the cell and the arrangement of its components is shown in Fig. 2. Its construction entails a number of problems. One arises from the lack of space in the objective air-gap and in the axis of the pole pieces, the over-all outer volume being no more than about 1/10th c.c. Furthermore, it is essential that the cell windows, despite their infinitesimal thickness, be strong enough not to give way when the complete assembly is introduced into the vacuum.

After a number of trials, we adopted a diameter of 0·1 mm. for the apertures O_1 and O_2. Under these conditions the force exerted by the internal atmospheric pressure on the surface of each window is less than 0·08 gm.

The components must be centred with great accuracy to ensure the fullest degree of coaxiality of the two cell windows.

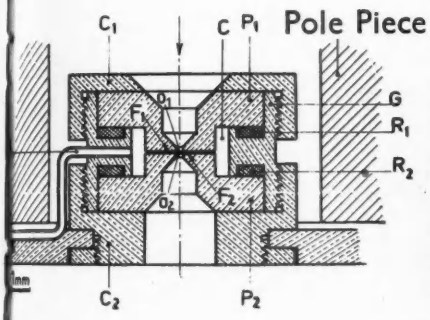
Progress of an experiment. Our early experiments were concerned with the observation of living bacteria. These were initially placed on F_1 or F_2, and sometimes on F_1 and F_2 together. With an optical microscope we were able to focus on each of the two windows to measure the thickness of the included air layer. The components of the object-cell were then assembled and the cell was ready for observation.

We made certain the bacteria did not die after a fairly prolonged stay in the cell by later punching out a piece of each supporting membrane and examining the specimen; the "punch" used was a 0·05-mm. tungsten wire. These trials showed that *the bacteria remained alive.*

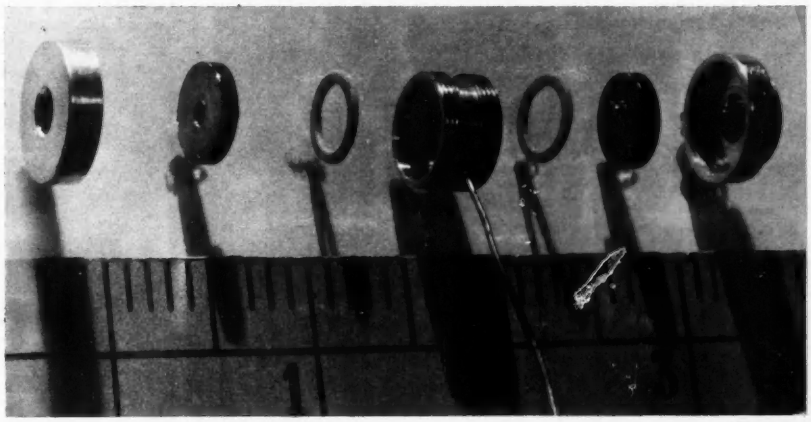
The observation and photomicrography of the bacteria within the object-cell was carried out with the electron microscope operating at the very high voltages referred to earlier; the exposure required for each photomicrograph did not exceed 2 to 3 sec. The diameter of the electron beam was slightly larger than that of the diaphragms O_1, O_2.

Although the bacteria used were subjected to high-energy electrons as well as X-rays produced by the impact of electrons around each diaphragm of the aperture, they *remained alive after irradiation and were capable of reproducing.* We worked with *Corynebacterium diphtheriae*, *Bacillus anthracomorph*, *Bacillus subtilis*, and *Staphylococci.* Further study will be necessary to find out if the prolonged effect of the electrons and of the highly penetrating X-rays they produce will result in morphological or genetic changes in the bacteria.

Some of the results obtained at voltages of 650 to 750 kV are shown on the following page. A and B are photomicrographs of *Corynebacterium diphtheriae.* C is of *Staphylococci*, and D shows sporulated *Anthracomorph* bacteria with the appearance of a nut-shell with a nut inside. These bacteria are generally quite opaque but some details of the internal structure can be distinguished and will become even more visible at a million volts.



Cross-section of object-cell.



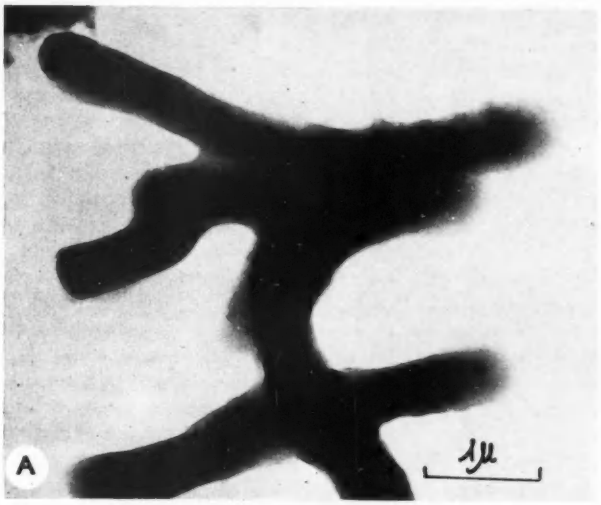
Photograph of components of object-cell. (*Photo Yan*)

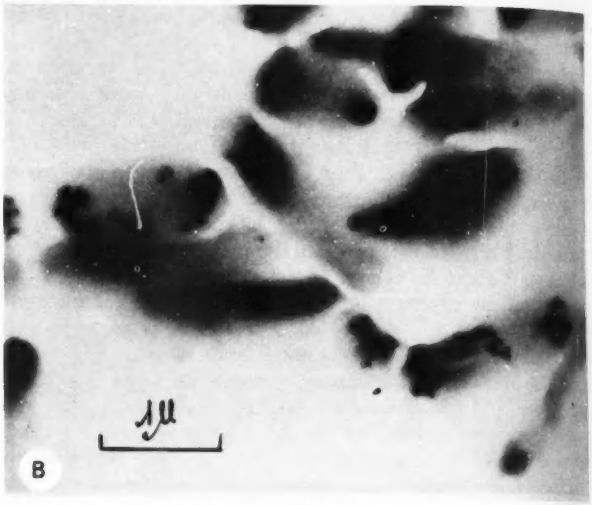
E is from a living diatom, a microscopic sea plant.

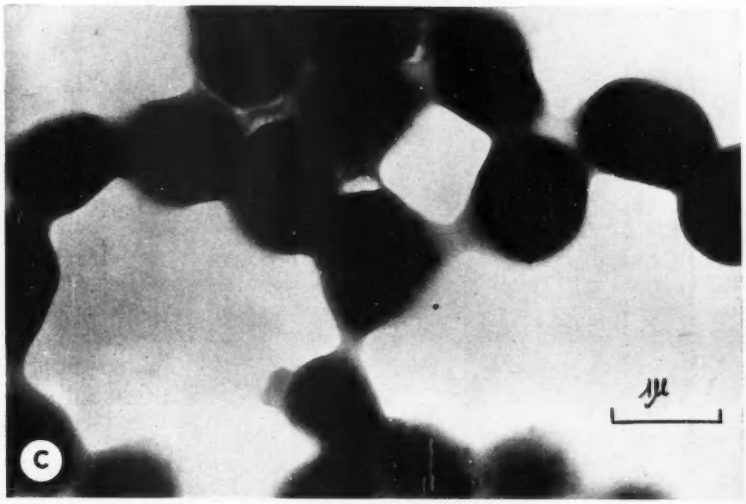
This work was carried out with the collaboration of the Bacteriological Laboratory of the Faculty of Medicine at Toulouse (Prof. Andrieu's department) which assisted us in providing study material, helping our technicians with the required precautions and testing the survival of the bacteria by reliable methods. We wish to express our special gratitude to Mme Enjalbert.

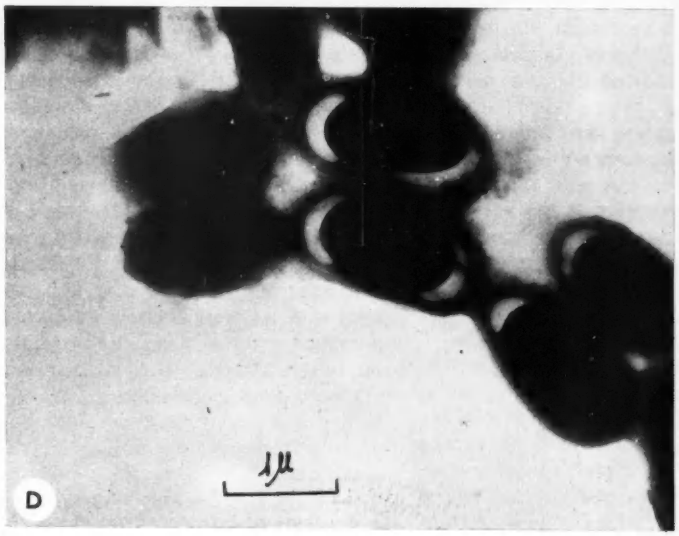
We are planning, shortly, to observe stages in the life of bacteria by taking photomicrographs at regular intervals over several hours. The method we have developed is expected to make a number of highly interesting investigations possible in biology and medicine.

This article was based on a paper originally published in the Proceedings of the Academy of Sciences (France) v. 251.









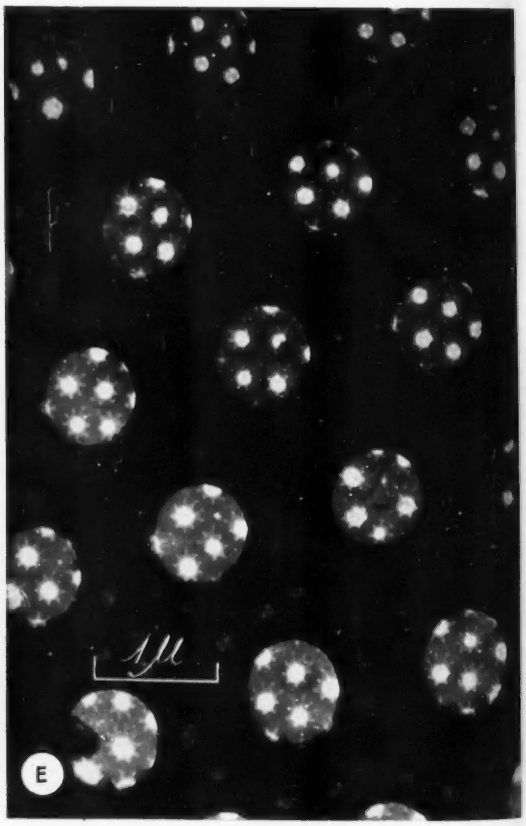
A, B. *Corynebacterium diphtheriae.*

C. *Staphylococci.*

D. *Bacillus anthracomorph.*

E. Diatom, microscopic sea plant.

Magnification of all photographs 17,000×.



THE BOOKSHELF

The planet Saturn as seen through telescopes of various sizes.

Outer Space Photography for the Amateur

By Henry E. Paul (*Amphoto, New York, 1960, 124 pp.*)

Astronomical photography is becoming a popular hobby, and this book, by a well-known amateur, is a welcome addition to the literature. It is not a textbook in any sense of the word; it is better described as a collection of "hints", based on the writer's considerable experience.

The opening chapters are general in nature. These are followed by a section devoted to "Telescopes, Mountings, and Drives", after which various classes of astronomical objects are dealt with in turn; stars, artificial satellites, the Moon, the Sun, nebular objects, meteors, and the planets.

The illustrations consist almost entirely of amateur-taken photographs. Some of these are by the author, but there are many others, including some remarkably fine pictures by the British amateur, H. E. Dall, E. C. Silva of Portugal, and Dr Clarence Custer of the United States. These photographs demonstrate the excellent results which may be obtained with relatively modest equipment.

The text gives some impression of hasty proof-checking, since it has been necessary to include an erratum slip and there are also some pen corrections by the author. Also, the lists of materials and suppliers relate mainly to the United States, and will be of limited value to British readers. However, these are relatively minor faults. The book will be found extremely useful, and has been well compiled; the author writes clearly, and his material has been selected with great care and skill. All those who have even a passing interest in amateur astronomical photography are recommended to buy it.

P. MOORE

Introduction to Theoretical Meteorology

By Seymour L. Hess (*Constable & Co. Ltd, London, xiv+362 pp., 60s.*)

Meteorology, perhaps more than most other branches of physics, is chronically short of good textbooks which may be confidently recommended to the student. In Britain, Sir David Brunt's classic, "Physical and Dynamical Meteorology", published as long ago as 1934, has not been superseded. American authors have

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been more industrious but not altogether successful, and so a post-graduate student coming new to the subject with a first degree in physics or mathematics has little choice between research monographs on specialised topics and rather stereotyped academic treatments which fail to meet the need and spirit of the times.

This excellent introductory text by Prof. Hess is therefore most welcome; it will undoubtedly fulfil a real need.

Having dealt with atmospheric statics, thermodynamics and radiation in a rather conventional but very clear manner, two-thirds of the book is devoted to an exposition of the essential ideas and basic facts of dynamical meteorology. Here Prof. Hess succeeds excellently in his declared aim of eschewing mathematical complexity in the interests of bringing out the underlying physical and mathematical concepts, and in producing a quite rigorous treatment which closely relates the subject to its basis in classical physics. The discussions of such topics as streamlines, trajectories, and the thermal wind are very well done. The author also puts some flesh on these bones with an up-to-date discussion on the general circulation of the atmosphere, which includes a clear account of momentum balance, mention of Fultz's model experiments, and the classical numerical experiment of Phillips. There is also a very readable account of numerical forecasting.

The general approach of this book may be a little unsatisfying to the more advanced and sophisticated student, but it will provide him with an excellent basis for further study and the beginner with a thorough grounding in the elements of the subject based on sound practical meteorological experience.

The quality of the printing, paper, and diagrams is excellent and the price reasonable. This book is recommended unreservedly.

B. J. MASON

Department of Meteorology,
Imperial College of Science and Technology,
London University

Arms and Insecurity
Statistics of Deadly Quarrels

By Lewis F. Richardson (*Stevens & Sons Ltd, 1960, xlvi+373 pp. and xxvi+307 pp., 63s. each, 115s. the set*)

Lewis F. Richardson, who died in 1953, made pioneering contributions to the study of meteorology by mathematical methods which were recognised by his election as an F.R.S. in 1926. For the last twenty years of his life, his main scientific interest was in the application of mathematics to the study of the causes of wars. These two books contain the most complete and developed account of this work. Parts of them have appeared in journals and microfilm manuscripts have been available for several years but the full texts have only now been published. It is not difficult to understand the reasons for this delay.

These are not textbooks with an obvious market; the subject-matter is an unorthodox field of scientific research and the techniques applied are unusual in social studies. A straightforward review which listed the contents of the books with comments on the appropriateness of the material and the lucidity of the argument would not be very instructive. In fact, problems are hacked at rather than mastered and the thread of thought is sometimes tortuous and far from clear.

Richardson's aim was to treat the occurrences and causes of wars by objective quantitative methods, which to him meant the development of mathematical models, that is, systems of relations between measurements that simulate real processes. All such models are simplifications and their value can only be assessed by their usefulness in explaining and predicting actual events. The core of "Arms

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and Insecurity" is a progressively elaborated mathematical model of the occurrence of wars as the consequence of an "explosive" armaments race. In its simplest form, the rate of change of the defences of each nation is taken to increase linearly with the menaces of other states but be reduced by rising expenditure. This leads to a system of simultaneous differential equations which, in certain conditions, can have solutions where there is an unlimited "explosive" increase in some of the menaces with time, taken by Richardson to imply war. The book examines the analysis of this system, extensions to allow for further factors, devices for the indirect measurement of the concepts, for example, by using armaments expenditure, and by applications to data for the periods preceding the two world wars.

The various strands in this theme are handled with unequal merit partly because of variations in difficulty. The most satisfactory section is the illuminating study of the effects of different levels of the controlling factors on the solutions of the equations. This shows the power of the method for exploring the consequences of given assumptions in complicated cases (where there are interactions among several nations) which could hardly be tackled by other means. On the other hand, the gauges for measuring the forces in operation, particularly the use of changes in reciprocal trade for "negative menaces" seem inadequate. The conclusions drawn from the fitting of the model to the empirical data are also unconvincing. Since the system is progressively evolved on the evidence of the observed measurements and allows a considerable degree of flexibility because of undetermined scale factors, it is not surprising that reasonable fits are obtained with the very limited material. Although the assumptions are plausible and the comparisons interesting, these are not adequate criteria of the usefulness of the model. Although Richardson was obviously deeply aware of the logic of applied mathematics, the understandable enthusiasm of the pioneer makes him overstate the weight of the evidence in favour of his theories.

In the second book, "Statistics of Deadly Quarrels", the aim is to find laws which describe the aggregate properties of wars, for example, distributions in time by number killed, number of nations on each side, and so forth, in terms of probability theory—that is, incorporating the effects of chance. About one-third of the book is an impressive compilation of materials for analysis on wars which occurred after the year 1820. On the whole the treatment is more straightforward than in "Arms and Insecurity", largely because it is more empirical, the measurements less arbitrary and the assumptions simpler. There is a brave attempt to develop models of the relationship between number of wars and opportunities for quarrels in terms of group sizes and contacts with potential belligerents. These are used to examine such questions as the aggressiveness of particular nations, and the pacifying influences of common languages, religions, and nationality.

It would be easy to criticise severely the studies in these two books. Treatments are sometimes inconsistent and often overelaborate; statistical tests are used (and sometimes misused) without consideration of the assumptions required to make them valid. More attention might have been given to the analysis of subgroups of data. Nevertheless, the work is full of thought-provoking ideas, and great ingenuity and persistence are shown in the devising of techniques for the attack on very difficult problems. These qualities are rarer than orthodox competence.

In order to gain a reasonable understanding of these studies an elementary knowledge of calculus and probability theory is required. Given this the books would repay reading by social scientists for the demonstration of the power and flexibility of mathematics in rigorous scientific inquiry, by statisticians for the illustrations of the methods required to extend the process of statistical generalization beyond the limits of immediate experimental data and by anyone for the convincing (and frightening) evidence of the importance of chance and uncontrolled elements in the occurrences of wars.

W. BRASS

Department of Statistics,
University of Aberdeen

Principles of Geochemical Prospecting

By I. I. Ginzburg, translated from the original Russian by V. P. Sokoloff (*Pergamon Press, 1960, xviii+311 pp., 70s.*)

This volume on geochemical prospecting for mineral deposits is based on the results of more than twenty years of geochemical prospecting in the U.S.S.R. Very little of this material has been available previously outside the Russian language literature. The volume of data available in the U.S.S.R. will be appreciated from the fact that in 1954–5 alone, 10 million geochemical samples were collected. In addition to Russian work, the book includes numerous references to research undertaken in the western world. There can thus be no doubt that the Ginzburg volume will fill an important place

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Editor M. W. THRING 50s.

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in the literature on mineral exploration.

As is to be expected, a large part of the data relates to the drier areas of the U.S.S.R. where so much of their geochemical prospecting has been undertaken. It is therefore most unfortunate when data from other parts of the world are misquoted. For example, on page 194, seasonal geochemical variations in Nigeria are correlated with the wrong seasons, presumably due to a mistranslation of the original English publication. Furthermore, it is not possible on this and other occasions to distinguish between Ginzburg's own comments on the data and those of the research workers from the text.

The translation from the Russian by V. P. Sokoloff is of a very high standard. Explanatory footnotes are given wherever there is any doubt as to the exact meaning of a word or phrase in the original Russian text. Terms such as "schlich", which have no exact equivalent in English, are explained in the footnotes when they first appear if their meaning is not readily apparent from the text. In a "Glossary" section, the translator gives a useful series of notes on soil terms used by Ginzburg.

The seventy-two diagrams have been redrawn and in almost all cases are clearer and more easily understood by the English reader than in the original. Unfortunately, however, as with most Russian publications, the maps often do not include a scale and this somewhat detracts from their usefulness.

Despite certain more or less minor blemishes, this book should appear on the shelves of all those interested in mineral exploration. However, the complementary volume, on the results of geochemical prospecting studies in the western world, is still sorely needed.

J. S. TOOMS

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Russian for the Scientist

By J. Turkevich, PH.D., and L. B. Turkevich, PH.D. (*D. Van Nostrand Company Ltd, 1960, ix+255 pp., 25s.*)

The aim of this book is to present an outline of Russian grammar in so far as it affects the scientist who wishes to translate technical literature and to introduce him to texts from different spheres of science. It consists of twenty lessons in which one, or more than one, grammatical topic is introduced, the majority of these lessons being followed by extracts taken from aeronautical engineering, biology, chemistry and physics. A series of appendices dealing with nouns, adjectives, pronouns, numerals, verbal forms and prepositions together with glossaries of English and Russian words complete the book.

The grammar is adequate to the extent that it covers the majority of points relevant to the translation of scientific literature but one could perhaps wish for more logical and concise presentation of the material. The introduction of more than one grammatical topic in most lessons may well be born of a desire to lead the student to connected passages at the earliest possible opportunity, an aim which is laudable in itself, but the selection of one grammatical subject for each lesson would have resulted in a greater concentration of effort on the part of the student. Noun endings are dealt with in seven different lessons—an unnecessary elaboration for the scientist. Each lesson contains numerous examples to illustrate a particular point. The passive voice, in view of its close connexion with participial forms, could have been considered together with the latter. Some aspects of grammar often encountered in scientific literature have been omitted, including "irregular" past tense forms of verbs, certain forms of comparison of adjectives and the special use of the Russian participle when it anticipates the noun with

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which it is associated ("participial sandwich"). There is no comprehensive list of conjunctions (there are a great number of them in Russian). A list of abbreviations used in science would have been a welcome addition too. There is a certain repetition of material in that *inter alia* noun endings and numerals appear both in separate lessons and as appendices in substantially the same form. There is also a tendency to stress irregularities. The note on chemical nomenclature might well have appeared as an appendix and this also applies to the summary of the uses of the various cases, each case being considered separately. The use of diagrams designed to offer relief from a mass of text is interesting but could have been made to yield a more varied vocabulary.

Each lesson includes a number of exercises containing sentences for translation from Russian into English. The former concentrates on the repetition of a limited number of words (a useful point), whereas the number of sentences to be translated into Russian is considerable. If a scientist can find time to do all these he will indeed have an adequate knowledge of the grammar.

The section on word building might well have been extended to include problems of consonantal and vowel changes as well as root words which are the key to the formation of word families.

The extracts themselves are followed by separate and adequate vocabularies and deal with a limited number of subjects. Aeronautical engineering appears to be the *leit-motiv* here. Whereas it is an extremely good policy to concentrate on a particular topic at a time when the students' main concern is with grammatical forms, a theme from either physics or chemistry—the structure of the atom, for example—might have proved to be a common denominator for scientists wishing to use the book.

The text is clear and pleasing to the eye and the book should prove to be a useful addition to those books designed to help the scientist evaluate Russian technical literature, though it is rather expensive in view of the limited number and nature of the extracts.

C. R. BUXTON
Department of Modern Languages, College of Science and Technology, Manchester University

Handbook of Abnormal Psychology

Edited by H. J. Eysenck (*London, Pitman Medical Publishing Co. Ltd, 1960, xvi +816 pp., £6.*)

This book is edited by Prof. Eysenck and consists of contributions by himself and eighteen past and present members of the Departments of Psychology and Psychiatry at the Institute of Psychiatry. The contents of the book are divided into three parts. Part 1 deals with the description and measurement of abnormal behaviour in terms of sensorimotor functions, perceptual functions, thought processes, and ego [*sic*] processes. Part 2 deals with the causes and determinants of abnormal behaviour, such as genetic factors, constitutional factors, conditioning and learning, childhood, and other environmental factors, and so on. Part 3 deals with the experimental study and modification of abnormal behaviour by methods of treatment such as drugs, psychotherapy, electroshock, brain surgery, and the like. The choice of contents has been governed by the research interests and prejudices of the authors. The former has led to the omission of certain important subjects such as old age and hypnosis and the latter for any discussion of the vast literature on the Rorscharch test; the authors exclude this because examination of this literature reveals that it is "all dross and no gold". Much dross has, however, been included and most authors are apologetic for their subject-matter—"A vast number of isolated, disconnected studies . . . the results of which are equivocal,

Organo-Metallic Compounds

G. E. COATES

So many important developments in the chemistry of organo-metallic compounds have taken place since the first edition was published just four years ago, that a revision was much to be desired. The rapidly increasing amount of published material has necessitated rather more selectivity in the preparation of the second edition, which, nevertheless, is about twice the size of the first, outgrowing the scope of the series (Methuen's Monographs on Chemical Subjects) in which it originally appeared. Most of the text has been re-written, and the largest amounts of new matter are in the sections on the alkali metals (including a brief treatment of aromatic hydrocarbon addition compounds), boron, tin, and in particular the transition elements.

The second edition retains the classification by Periodic Group used in the first edition, as this arrangement makes for easy reference. The treatment is not intended to be comprehensive, but rather to be suitable for undergraduates or for research workers seeking an introduction to the subject. Valency problems, for example those concerning metal-hydrocarbon π complexes, which were mentioned only very briefly in the first edition, have been discussed at much greater length in the present work and with the object of making the subject comprehensible to those not already familiar with molecular orbitals. 45*s*.

The author is Professor of Chemistry in the University of Durham.

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METHUEN

inconsistent, and very often contradictory". This apologetic attitude is also shown when Jones writes of "the low status of psychology as a scientific discipline, both in terms of its reputation among representatives of other disciplines and in terms of actual achievement as represented by a coherent body of knowledge". Contrasted with this is the apparent precision of the same author's modification of the basic Hullian equation.

In such a large volume it would be invidious to select individual contributions for special mention. The motif of the book is Eysenck's theory of personality and although Franks admits of inhibition which is central to this theory ("It is impossible . . . to define without embarking upon considerable and highly dubious speculation of a neurophysiological nature") he defends the concept vigorously, calling criticisms of various aspects of Eysenck's theory "jejune" and "chimerical". There is, indeed, in this book a curious mixture of the humble and the arrogant.

Of lesser importance is the style, and of this it need only be said that much of the book reads like regulations of a government department. It is as though the authors, like Zola, are determined to show that if they have not got genius, they can at least be dull. There are exceptions, notably the chapters by Shields and Slater on Heredity, by Rees on Constitutional Factors and the contributions by Eysenck himself.

This is a major work and despite all its faults, there is no comparable book in English. A vast amount of valuable information has been collected and an excellent index facilitates the use of this volume as a work of reference. It is certain that further editions will be called for and one hopes, with the editor, that in the second edition "the publishers will be less insistent on our not overstepping the total number of words allocated to this venture".

A. W. BEARD

Department of Psychological Medicine, The Middlesex Hospital, London

Radiation Protection and Recovery

Edited by Alexander Hollaender (*Pergamon Press, 1960, v+392 pp., 70s.*)

One of the most striking recent developments in radiobiology has been the clear indication that effects of ionising radiation observable at the cellular level—particularly on somatic cells—can account satisfactorily for many phenomena seen after the irradiation of a biological entity as complex as a mammal. Certain firmly based principles, of great relevance to the subjects of this book, apply to all cells, from micro-organisms upwards. The expert radiobiologist, conscious of these principles, will not experience the need to have them explicitly stated and will regard this collection of articles by radiation research scientists as reviews of recent literature, and, in many instances, of their own work; the majority of the contributors are members of the Biology Division at Oak Ridge, of which the Editor is Director. The reader who is not very familiar with the subject will, however, suffer from disadvantages which are perhaps inherent in this technique of scientific bookmaking; the material is not co-ordinated, and is split up according to the fields in which the individual contributors work, instead of being arranged in a logical sequence which would give shape to what must appear as a confusing picture. As a result, one important aspect of protection, the influence of oxygen concentration at the time of irradiation, has had to be discussed independently by almost every author; while the growing volume of work on dose relationships for the killing of somatic mammalian cells, which has become an outstandingly important branch of radiobiology, has escaped mention altogether.

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time of radiation enhances damage or, conversely, that its removal protects, is one of the most generalised phenomena of radiobiology, applying to almost every type of cell and every test of damage. The importance of anoxia as a protective mechanism is rightly stressed in the editor's brief introduction, and most of the contributors acknowledge that some chemical protective agents operate by inducing anoxia in one way or another. It might be questioned, however, whether all the implications of the oxygen effect have been fully appreciated by some of the authors; for example, that a change of oxygen gradient will occur in organised tissues if there is a change in the respiration rate of cells through which oxygen diffuses. This and other important manifestations of the oxygen effect have been well substantiated, and it is unfortunate that P. Alexander, in his opening chapter, did not refer to these aspects of chemical protection in his brief discussion of protective mechanisms *in vivo*. His treatment is presumably intended to suggest relationships with observations made on macromolecules, but the statement that the "oxygen hypothesis" must be limited to autoxidisable substances could be very misleading.

The effect of respiration rate on oxygen gradients also has a bearing on variations of radiation response with temperature in certain test systems, a fact which is mentioned only by Conger in his admirable review on genetic effects. Wolff and Whiting, however, do not mention this effect of temperature but discuss only the change in solubility of oxygen which, as Wolff rightly observes, cannot account for the whole of the influence of temperature on radiation effects observed in some organised tissues. Omissions and inconsistencies of this nature suggest that the editing might have been stricter, and even that an explicit chapter on the ramifications of the oxygen effect should have been included in a book largely devoted to the subject of protection.

In its radiobiological sense, "Protection" means a modification in environment at the time of irradiation which will diminish whatever effect is being studied. Cellular effects can account for numerous observations on tissues and whole organisms, and to that extent the various chapters of the book are linked together when they deal with protection. The term "recovery", however, is used in radiobiological research generally, and in this book, with different meanings in different contexts. Stapleton, for example, when writing about recovery of bacteria and fungi, is concerned with post-irradiation techniques which modify the response through their interaction with biochemical processes within the cell. There is as yet little evidence of analogous techniques which can similarly modify the response of somatic mammalian cells. In a somewhat different sense, there is a manifestation of "recovery" in mammalian cells which has great importance for radiotherapeutic techniques, but this discovery is perhaps too recent to have been included in the book. The "recovery" of irradiated animals, however, is something more akin to the medical use of the term; specific treatments, notably the grafting of haemopoietic tissue, will enable an animal to survive an otherwise lethal dose of radiation. Recovery in that sense is dealt with in the chapters by Smith and Congdon on whole-body radiation injury in mammals, by Makinodian and Gingozian on antibody formation, and by Odell, Cosgrove, and Upton on delayed somatic effects. These three contributions will be valued for providing extensive discussions and bibliographies concerning an aspect of radiation research which has developed at a very high rate within the last few years.

TIKVAH ALPER

Experimental Radiopathology Research Unit, Medical Research Council

Current Problems in Animal Behaviour

Edited by W. H. Thorpe and O. L. Zangwill (*Cambridge University Press, 1961, xiv+424 pp., 45s.*)

This book arose out of a series of discussions at Cambridge over a period of years, 1953–8, between zoologists concerned with animal behaviour and psychologists. This interdisciplinary contact at first involved difficulties because of the different approach of the two sides, the zoologists commonly preoccupied with species differences and evolution, the psychologists with mechanisms and learning. But when it became clear that both groups were often studying the same problems, their points of view were seen to be complementary and a help rather than a hindrance to a better understanding, as the book itself testifies.

Fourteen members of the discussion group prepared papers on topics of interest in the study of behaviour as a whole, each contributor choosing his own subject. These papers were then read and discussed by the group before being drafted in their final form for publication. While such a method cannot produce an even coverage of the whole field of animal behaviour, this is no criticism of a book which fully justifies its modest title. It is only regrettable that the essays, which were written three or four years ago,

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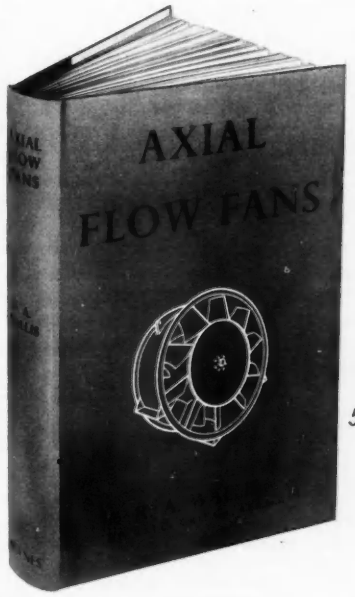

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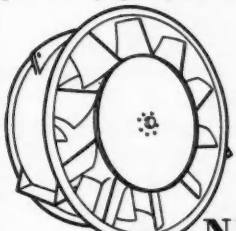

NEWNES

should not have been published until now.

The book is divided into four not very clearly defined sections, each with its own introduction by one of the editors under the headings Neural Mechanisms and Behaviour, Experimental Studies of Animal Behaviour, Studies of Problems Common to the Psychology of Animals and Men, and Theoretical Approaches to Behaviour.

To select particular topics for mention may reflect more about the reviewer than the book, but one can refer in the first section to Vowles's discussion of the differences between the nervous systems of insects and vertebrates, and the consequences of this for the co-ordinating mechanisms which each have evolved to achieve the same kind of behaviour. Zangwill re-evaluates the classic work of Lashley and others on the specificity of the cerebral cortex of mammals for learned behaviour.

The second section contains one paper on ritualisation (Blest) and another on releasing mechanisms (Marler), concepts which have been much used by ethnologists in connexion with the original Lorenz-Tinbergen scheme of behaviour. The present contributions show how the concepts can be used, if necessary, outside the theoretical framework in which they were first conceived, and review the bearing of more recent work upon these ideas.

Among the third group of papers are two (Hinde, Thorpe) which arise out of work on "imprinting". It is shown how this apparently peculiar kind of learning shares many features with, and throws light on, more conventional learning, while its special properties would seem partly fitted to the early age at which this process takes place and the habits of the species in which it is found. The classical imprinting, by which a young bird learns to follow its mother, raises problems for the learning theorist of what reinforces the following behaviour, and a similar problem is raised by the song-learning of birds. This whole question of reinforcement is considered more generally in a separate review by Watson, who points out the limitations of the "drive reduction" hypothesis.

Two of the last essays (Gregory, Broadbent) argue for the cybernetic approach to behaviour and show how this method can be valuable in rather more concrete terms than is sometimes done. Indeed, Gregory argues that the kind of experiments of stimulation and ablation usually used to study the localisation of brain function are almost bound to be incomprehensible until one has for the brain some kind of "block diagram" (of a kind familiar to engineers) in which the functional, as opposed to anatomical, units of the brain are first identified and the relation between these units established. How to construct such a diagram remains a problem, especially if, as he suggests as a possibility, some quite new principle is involved that is as unfamiliar to us today as the idea of a feedback loop before Clerk Maxwell.

J. M. CULLEN

Department of Zoology and Comparative Anatomy, Oxford University

An Introduction to Transition-Metal Chemistry—Ligand Field Theory

By Leslie E. Orgel (*Methuen, 1960, 180 pp., 25s.*)

This work is an attempt to apply the ligand field approach to a study of many aspects of transition-metal chemistry. The mathematical considerations leading to the various theories have been completely excluded from the book and the general results applied. This approach will prove extremely popular to many experimental chemists, as no simple consideration of this type is available elsewhere. As Dr Orgel points out, the book will be of most use when it is used in conjunction with a work on the mathematics of the subject.

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The book is intended to be a complementary volume to the theory of Transition-Metal Ions by Dr J. S. Griffith. However, the main readers of the work will certainly be interested only in the results of the ligand field theory and it may have been of more general use to have extended this work to briefly enunciate the mathematical propositions on which the theory is based.

Dr Orgel has certainly fulfilled a long awaited need, and the general principles of ligand field theory are simply stated and applied to a number of subjects dealing with Stereochemistry, Spectra, Reaction Mechanisms and the Stabilisation of Low and High oxidation states of transition metal ions. In particular, the chapter dealing with ionic and co-valent bonding is a very good exposition of what is to many students a difficult problem in transition-metal chemistry. Although alternative interpretations are referred to throughout the text, the one danger of the book in the hands of students may be the tendency to apply the theory to problems without the critical mathematical background necessary. There is little doubt that this will be an extremely popular book and will be of interest to all students of inorganic chemistry. The book is well presented and the only minor mistake noted in the text was the references to the stabilisation of metals in low oxidation states by ortho-phenanthroline rather than by dipyridyl.

J. LEWIS

Department of Chemistry,
University College, London

Radioactivity for Pharmaceutical and Allied Research Laboratories

Edited by Abram Edelmann (*Academic Press 1960, xii+171 pp., $6.00*)

There can be few industries that use so many scientific disciplines as the pharmaceutical industry; consequently, any examination of a technique based on a phenomenon such as radioactivity in fields as remotely related as chemical engineering and medicine will probably arouse interest. Such an examination is attempted in this book. The title is misleading for it is disappointing to find the contents a collection of papers presented at a symposium, organised albeit creditably by the Nuclear Science and Engineering Corporation of America. Two of the papers, though advantageously included, deal with sterilisation by electron bombardment and a specialised application of x-rays—techniques not normally regarded as applications of radioactivity. That on sterilisation by Van Winkle is a frank, informal and interesting account of the successful application of Van de Graaff and linear accelerators to commercial sterilisation of prepacked sutures, whilst Charipper and others report on the effect of drugs (some of which are unidentified chemically) on the repair of haematological damage caused by controlled doses of x-rays.

The remaining papers are concerned with the use of radioactive isotopes, and are of variable quality. The least satisfying is the one by Christian on principles, methods and utilisation; it contains no references and a number of misleading generalisations, for example: "Another distinct advantage in the use of radioisotopes is the ability to detect ionising radiation at a distance, and through substantial thicknesses of material."

In contrast the papers by Donia and Snell are informative and good examples of the way to combine this technique with others in research. Snell describes the use of C^{14} in work on the antibiotic oxytetracycline (Terramycin). His studies on precursors (to obtain the labelled material), distribution and transport, as well as effects on bacterial cell metabolism, pose interesting questions concerning the significance of serum levels and biosynthesis of this drug.

Examples of analytical procedures are given by Donia and Craig. The former's is for sterol analysis and leads to a new extraction procedure for a naturally occurring sterol, whilst the latter's deals with an insecticide and an antibiotic. Both make comparisons with other analytical procedures.

The adsorption, distribution and excretion of reserpine is studied a little inconclusively by Sheppard; nevertheless it illustrates the problems of interpretation of results and labelling. Bogner's review on the related subject of development and evaluation of drugs is useful and constructive. Similarly Nelson reviews some applications to research on detergents and cosmetics. His references range widely and include a pertinent example of misuse of the technique.

The final paper on the Isotope Development Programme of the American Atomic Energy Commission is informative but leaves an impression that isotopes must be increasingly used at all cost. In this connexion particularly, the absence of a paper on methods of handling and disposal of labelled material is regrettable since many readers will be novices. Similarly, a paper on instrumentation and a short bibliography for further reading could have been included.

Subject-wise, the collection of papers is, for a short symposium, reasonably balanced. Nevertheless, the editor could have improved matters by not confining his main contribution in the first paper to a simple, though constructive, account of the use and effect of ionising radiation.

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H. D. C. RAPSON

Physical Chemistry Department, Beecham Research Laboratories Ltd

Introduction to a Sub-molecular Biology

By A. Szent-Györgyi (*New York, Academic Press, 1960, x+135 pp., 40s.*)

This little book, which some will find stimulating and others infuriating, is about electronic excitation processes in biology. The importance of such processes in photosynthesis and probably also in oxidation-reduction reactions in living tissues is indisputable and Prof. Szent-Györgyi offers not very clearly defined suggestions of their importance in other connexions. These may well stimulate some thought on these subjects but it is difficult to know to whom the book is addressed. It is written in a fairly popular style, yet it seems to me that it would be unintelligible without a good deal of knowledge. On the other hand the expert would like the ideas to be defined and worked out much more precisely, Indeed, he would probably get a much better idea of the status of this subject by reading the papers presented at two recent symposia which have included this topic, namely The Faraday Society's general discussion on energy transfer in biological systems (1959) and the Brookhaven Symposium on bio-energies (1959).

Szent-Györgyi's suggestion that the electronic properties of important biochemical compounds may provide explanations of their behaviour deserves full consideration. It is quite true that biochemists have been content to find the chemical formulae of a great many potent substances without asking *why* they have these peculiar properties. However, in many cases the substances do not appear to be extraordinary as chemical compounds, and it must be remembered that the compound is only part of the reacting complex; for example, we are not likely to understand how a simple chemical hormone works unless we know what systems, such as proteins, it reacts with.

It may also be the case, as Szent-Györgyi suggests, that the peculiar properties of proteins are connected with a special pervading electron structure, but it has been difficult to find evidence of this and it has not been shown at present that simpler "chemical" explanations are inadequate. For example, how are we to explain the remarkable and highly specific catalytic properties of an enzyme? Only now are the chemical structures of the enzymes as a whole and their active centres

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being arrived at; it is difficult to say whether considerations other than those involving the close fitting together of the substrate and the enzyme and forces such as are provided by hydrogen bonds and the concentration of charged ions at particular points will be required. At the moment further speculation is rather barren.

Other very brief chapters deal with such topics as the role of ATP in vital oxidative processes, mechanisms of drug action, and characteristics of the living state. These all involve numerous unsolved problems and Szent-Györgyi does a service in mentioning possible new approaches, although he clearly is not under any illusion of having arrived at solutions. I believe his main object is to make people think about these problems and even if he often leaves us a bit puzzled about his arguments, there is no doubt that this end is achieved. J. A. V. BUTLER

Professor of Physical Chemistry,
Chester Beatty Research Institute

Methods in Geochemistry

Edited by A. A. Smales and L. R. Wager (*Interscience Publishers Ltd, London, vii+464 pp., 85 text-figs., 18 plate figs., 94s.*)

With the rapid advance of geochemistry in the last decade there has been a growing need for a concise account and assessment of the analytical methods available to geochemists. This book fulfills that need very well. A short introductory chapter by the joint editors is followed by an account of the collection and preparation of material for analysis (by L. R. Wager and G. M. Brown). Sampling techniques, precautions against extraneous contaminative and mineral purification methods are described. Those geochemists with a background in physics rather than geology will appreciate the stress the authors give to the importance of initial sample selection.

The well-established analytical techniques, including gravimetric, volumetric, flame photometric, and colorimetric methods, are presented in chapter III (by E. A. Vincent) with an excellent section on precision and accuracy. The emphasis is on the determination of the normal major elements in silicate rocks using both classical and rapid methods, but examples of the application of colorimetric methods in determining thirty of the rarer elements are tabulated. Chapter IV is on spectrochemical methods (by S. R. Taylor and L. H. Ahrens) and includes an account of the accuracies and precision experienced in determining both major and minor elements. Detection limits for seventy elements are tabulated adjacent to respective values for crustal abundance of the elements and it is clear that the optical spectrograph will remain a most useful geochemical tool especially for the minor elements. The detection limits listed for the rare earths are presumably for isolated rare earths when the special problem of line coincidence due to other rare earths is avoided. Rare earths are almost invariably associated with other rare earths in nature and these detection limits are therefore geochemically misleading. Chapter V is a useful account (by H. I. Shalgosky) of fluorescent x-ray spectrography with a summary of its advantages. The geochemical applications which are described include the estimation of the two important pairs of elements Nb and Ta and Zr and Hf. Optical spectrographic methods are not well suited to the determination of either Ta or Hf and it might have been mentioned that optical and x-ray analytical methods are to some extent complementary. Chapter VI, on isotope geology (by K. I. Mayne), includes a valuable section on mass spectrometric principles and practice. Estimation of the natural variation of the isotope of O and S is discussed as are dating methods with reference to K/A,Rb/Sr, U and Pb. Errors due to the treatment of sample, contamination, instruments, etc. are discussed in the subsequent chapter (by R. K. Webster) on mass spectrometric isotope dilution analysis. At least four-fifths of

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the elements may be estimated by the latter technique but its main geochemical application concerns age determination using the elements just mentioned. It might have been preferable to have combined chapters VI and VII as they hinge on the use of the mass spectrometer. Chapter VIII on radiochemical methods (by S. Moorbath) deals incisively with instrumentation needed to detect natural radioactivity. Applications include the determination of radioactive C, Cl, A, I, Rb, and Th and U and their decay products. The use of radioactive tracers is also briefly mentioned.

Chapter IX, on radioactivation analysis (by Dr Mapper), is of particular interest to geochemists who lack expensive equipment like optical, x-ray or mass spectrometers. Only relatively inexpensive counting equipment is needed (discussed in chapter VIII) together with a moderate budget to defray irradiation costs. Many of the recent geochemical applications are cited including the determination of Cr, Co, Ni, Cu, Ga, Ge, As, Rb, Sr, Pd, In, Sb, Cs, Ba, Re, An, Th, and U in meteorites or rocks. A table (previously published) shows the estimated sensitivities of sixty-eight elements in radioactivation analysis. Presumably the omissions indicate the unsuitability of the method (that is, V, Nb, Rh, Pb). Polarography is described in chapter X (by S. Moorbath). The potential use of this technique has been known for some time yet geochemical applications remain rather few (that is, Ti, Co, Ni, Cu, Cd, and U). One reason is that sensitivities obtainable fall short of those experienced for some elements in optical spectrographic analysis and, especially, in radioactivation analysis. Application of the square wave polarograph will remedy this but the snag of reagent contamination (absent in radioactivation analysis) will remain. The final chapter deals with some modern chemical separation methods (by F. W. Cornish), including ion exchange and chromatography. These methods may, of course, be useful as purification steps in colorimetry, polarography, or radioactivation analysis. Nevertheless, few of the 142 references have much geochemical flavour.

J. R. BUTLER

Department of Geology,
Imperial College of Science
and Technology,
London University

Animal Behaviour

By J. L. Cloudsley-Thompson (*Edinburgh and London, Oliver & Boyd, 1960, xiv+ 162 pp., 15s.*)

This book arose from a series of London University Extra-Mural lectures, in which the author, as he writes in his preface, has attempted "to explain simply the modern approach to the study of animal behaviour".

The book does indeed describe in simple language a great many instances of behaviour of quite a variety of animals from Protozoa to Mammals, including Man. He also discusses briefly the results of some analyses. The book is richly illustrated, and a short reading list is provided. The variety of behaviours mentioned and the numerous illustrations will undoubtedly attract a number of readers, and the book may thus stimulate interest in this already popular field.

However, it can hardly be said to "explain . . . the modern approach to the study of animal behaviour". Dr K. Mellanby says in his foreword that ". . . the facts are allowed to speak for themselves . . ." and this may be the clue to the rather unfortunate shortcomings of the book. Admittedly, facts can be made to "speak for themselves", namely when they are selected with care and are ordered in such a way as to suggest the lines of thought and the conclusions reached. Such organisation of the facts can of course be achieved according to a variety of principles. One could for instance arrange the material according to major functional

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systems such as feeding, protection from predators, reproduction, etc.—or one could arrange it according to the nature of the physiological problems encountered—or one could proceed from the simple to the more complicated—or behaviour could be treated with reference to ecological or taxonomic types, etc. But the author has hardly ordered his material at all. The first chapter on "Survival in a Hostile World" begins with five pages of rather irrelevant remarks on the origin of the Universe ("It is well to start at the beginning") and of life on Mars. That on "Life on Land" is followed by one on "Nature by Night" and this is succeeded by one on "The Language of Animals", after which we get "Feeding and Fighting".

Within the chapters the treatment often jumps from one subject to another. What the author does is to make the reader *browse*, but instead of allowing his herd at least a couple of minutes on any one spot, he drives them on, impatient to show them something new even before they have had time to sample more than a few shoots. This would perhaps not matter so much if at least the shoots were tempting and nourishing. But although many interesting types of behaviour and problems of function and causation are touched upon, the treatment is often superficial and sometimes unintelligible.

On page 63, after we have just heard (page 61) that "releasers are often affected by imprinting", we read that "Animals of different species are really never able to make friends with one another because their different languages are instinctive and cannot be learnt", and yet, in the same breath: "nevertheless [*sic!*] . . . they misunderstand each other at times."

On page 102 we read: ". . . time-compensated solar navigation has been established . . . in birds", but on page 106 it says: "What determines the direction of migration (in birds) is . . . unknown. It has recently been suggested . . . that birds navigate by means of the sun's arc which is estimated visually." Unless the reader is really well acquainted with Kramer's and Matthews' work he will not understand what this means.

In my opinion books such as this, kaleidoscopic and superficial, do not render a service to the popularisation of biology. Non-scientists will be confused, fellow-scientists working in other fields will lose interest in a rapidly maturing branch of science, and the specialist in animal behaviour will be annoyed by the poor showing of his field of research.

N. TINBERGEN

Department of Zoology, Oxford University

Nuclear Structure

By Leonard Eisenbud and Eugene P. Wigner. Investigations in Physics, Vol. 8 (*Princeton, NJ: Princeton University Press; London: Oxford University Press, 1958, viii+128 pp., 25s.*)

The current profusion of research into the various aspects of nuclear physics presents a formidable problem to the textbook writer, for it is all too easy for simplicity to be tangled in a web of detail and for obsolescence to precede publication.

In these circumstances, it is refreshing to find that the book under review has lost little of its value in the three years which have elapsed since it was first published—a feature which is brought about to a great extent by the authors' avowed concentration upon useful points of view, which have endured, rather than details which may have foundered; and, indeed, except for one, albeit important, topic, recent progress would not call for extensive rewriting, even today.

A compact but remarkably comprehensible account of the main properties of nuclei, low energy reactions, and nuclear theory is supplemented by a very useful guide to selected references and is presented with little recourse to mathematics so that the book is, for the most part, accessible to anyone who has an understanding of the physical basis of quantum mechanics, yet no great facility in technical manipulation.

The early chapters of the book review the well-established features of nuclear binding energies, decay processes, spins, magnetic moments, radii, and reactions. There follows a very brief account of our knowledge of nuclear forces, which is sufficiently modern to make reference to repulsive cores but precedes the recognition of the spin-orbit potential.

A very reasonable emphasis is placed upon independent particle models, yet the collective model is not neglected and future developments are anticipated by making mention of progress in the establishment of approximate methods of solving the nuclear many-body problem. The theory of nuclear reactions is presented in a clear but penetrating manner but the chapter on β-decay is one of the many casualties of the recent revolution.

At many points, the brevity of the discussion may well embarrass the beginner but anyone with some acquaintance with nuclear physics who is in search of a source of perspective will find it here.

V. J. EMERY

Department of Mathematical Physics, Birmingham University

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LETTERS TO THE EDITOR

Science and Art

Sir:

None would quarrel with the thesis that, if the general public is to understand and appreciate the scientist, he must find a way of communicating scientific ideas in intelligible terms. However, it is to be doubted that the Skeaping Frieze (DISCOVERY, 1961, vol. 22, No. 2, p. 45) makes a contribution in this matter.

You, sir, find it useful to explain the symbolism; I, a scientist, by training and profession find the explanation necessary because so little of the symbolism meant anything to me. How much less then will it mean to the layman? The Frieze, it seems to me, is one more example of the obscurantism and mystique with which some scientists give the impression of seeking deliberately to blind with science.

H. GARDNER

Camberley, Surrey

Television in the Science Classroom

Sir:

We have read with interest the article, "Television in the Science Classroom" by Hugh Pitt, in your December edition. There is, however, one statement made by Mr Pitt that has caused us concern.

Under the sub-title "Evaluation" he states: "Granada does not place quite the same emphasis on advisory bodies, reports, and so on, but does arrange conferences with schools and education authorities, and takes the advice of teachers' associations and kindred organisations."

This is in fact entirely wrong and we would be pleased to know the source of Mr Pitt's information.

Granada has a Schools Television Advisory Committee consisting of the Chairman, Prof. Mansfield Cooper, Vice-Chancellor of Manchester University, and seven members nominated by the leading educational bodies. This committee meets several times a year and its approval is required before any schools programme is produced or transmitted.

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GERALD BARRY

Granada TV Network Limited, 36 Golden Square, London, W.1

Sir:

Getting any information from Granada TV Network Ltd, as opposed to the BBC or Associated-Rediffusion, in regard to their schools programmes proved quite difficult and, as they apparently have no Head of Schools Television, I was eventually referred to their Press Office.

As a result, all that was reported in my article was the outcome of discussions with that department, and no mention was made of the distinguished names in the above letter. Had these names, and the other facts now pointed out by Granada, been brought to my attention, these points would, obviously, have been included in the article.

Incidentally, throughout my search for someone to talk to me about Granada's television for schools service, I was told, both in London and Manchester, that Sir Gerald Barry was "not available". H. PITT

Russian for Scientists

Sir:

As the authors of the book "Translation from Russian for Scientists", which was reviewed in the February issue of your journal, we would like to make the following comments.

The reviewer has spent a considerable part of his time considering the question of "accents" and deplores the absence of them in the book. The question of accents or stresses, as they are normally called, is an important one in books which are to assist in learning *spoken* Russian. The main purpose of our book is to enable students to understand and *translate* Russian scientific periodicals and they will certainly find no accents or stresses appearing in them—a fact which will not hinder the attempt at translation. The reviewer seems to have approached the subject from the point of view of pronunciation and not from that of translation into English.

Moreover, in criticising the amount of reading material provided, the critic has totally misunderstood the aim of our work, which is to provide a large and varied text for those who have no access to Russian scientific literature and periodicals. C. R. BUXTON, H. S. JACKSON

Department of Modern Languages, Manchester College of Science and Technology

Vortex Down Under

Sir:

On a recent trip to the antipodes, I was asked to investigate a topic which has interested the non-ferrous metals industry for some considerable time: the direction of rotation of the water in the domestic bath as it runs down the drain.

I considered that I could possibly assist because I had given some study in recent years, as an erstwhile physicist, to the non-equivalence of parity and the paradox of the right- and left-handed universes. It seemed to me that a similar approach could be applied to the problem which has exercised the copper-tube trade, particularly that part of it which is concerned with the manufacture of domestic water service fittings (DWSF).

The common theory is that the vortex formed by the water rotates clockwise (c.) in the northern hemisphere and anticlockwise (a.c.) in the southern hemisphere. This direction of rotation could have a close connexion with corrosion in copper pipes. The subject was obviously of importance, since we already make DWSF in Australia and are proposing to erect a factory to manufacture copper tubes in New Zealand. If the spiral motion of the water is reversed in the antipodes, it may be that the corrosion which occurs on the inside of tubes in Britain may be reversed to occur on the outside in New Zealand: this poses some very ticklish problems for the technical people.

Having agreed, at short notice, to carry out some researches during my trip, I naturally wished to make some first-hand observations in the U.K. before departure, and I initiated the research programme

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with enthusiasm. Apparatus for such work, I soon determined, was not costly. The best method of making observations is to fashion small paper boats, as children do, from a supply of non-absorbent paper, which is readily available, and place them near the edge of the whirlpool if and when it forms.

Like charity, which begins at home, I started *chez moi*. Results were inconclusive, since my bath water hardly runs out at all, and certainly not in a spiral movement. In order to save the situation, as I thought, I made a quick trip to Yorkshire Imperial Metals, Leeds, the mecca of DWSF, so to speak, but the hole in the hotel bath was of such large diameter that the water ran out vertically, and there was no vortex either. The Boeing 707 in which we made the trip to Sydney, via America and Honolulu, didn't further my research, since there was no bath and no future in "topping and tailing" in a small basin at 35,000 ft.

After thirty-two hours' flying I reached Sydney in fairly good shape, but tired and ready for a bath—not one bath but many. Eleven ran out with a clockwise spiral, and three started clockwise and finished anticlockwise. In the absence of a Research Managers' Conference, I found this a bit perplexing.

Two days later we went to Melbourne, but I'm sorry to say that, like so many research chemists, I lost my piece of paper and, having failed to retrieve it, I cannot remember which way the spiral went. It was with added zest that I took the next leg of the trip to New Zealand, where I looked forward to seeing the site of the new factory and to catching up with my research programme. Unfortunately, some non-technical type must have arranged the accommodation and provided me with a shower. Time did not allow a trip to Rotorua to ascertain whether the hot springs ran back into the bowels of the earth c. or a.c.

Back in Australia we spent a day at a cousin's "station" at Gunnedah, north-east of Sydney. I don't think the station is beyond the "black stump", because there are baths there, not "Alice Springs", and the water ran out clockwise not once, but twice. Melbourne wasn't a write-off, however, because we went back again to see ICI House, and that would be something, even in Texas. Twice the bath ran out a.c. but once a.c., followed by c. towards the end.

On the return flight home we stopped off at Singapore and had two baths: the first was a.c./c. and the second c. all the way. At Kuala Lumpur (KL) the score was three a.c./c. and one c. all the way. Karachi yielded three c.'s and in revolt three c./a.c.'s. At Khewra Soda Factory we were provided with the biggest bathroom I have ever seen. The egress of the bath water was c./a.c.

On arriving in the U.K. I decided to brave the cold and the wet to make a pilgrimage to Scotland, where folks are logical and lost causes are considered with serious courtesy. I was tired, and I had a dose of 'flu coming on, due to excessive bathing, having removed the fats and protective vitamins from my epidermis; but I was determined. It was my last visit to Nobel's as Group Director, and I resolved to go back to London cleansed in a last burst of exact and quantitative bathing. At the Royal Scottish Automobile Club, which had meant so much in my motor-rallying days, I proudly pulled the plug, not once but twice, and got two magnificent a.c. spirals all the way. At the farewell dinner at Glenfoot the Nobel Division gave me a silver cigarette-box and two baths, both clockwise. That decided me. In future I'm leaving research to the Research Director, and as soon as he has finished his present investigations on throwing eggs over house-tops to see if they break I hope he will take up the spiral movements of liquids from venting vessels.

JAMES TAYLOR

Yorkshire Imperial Metals

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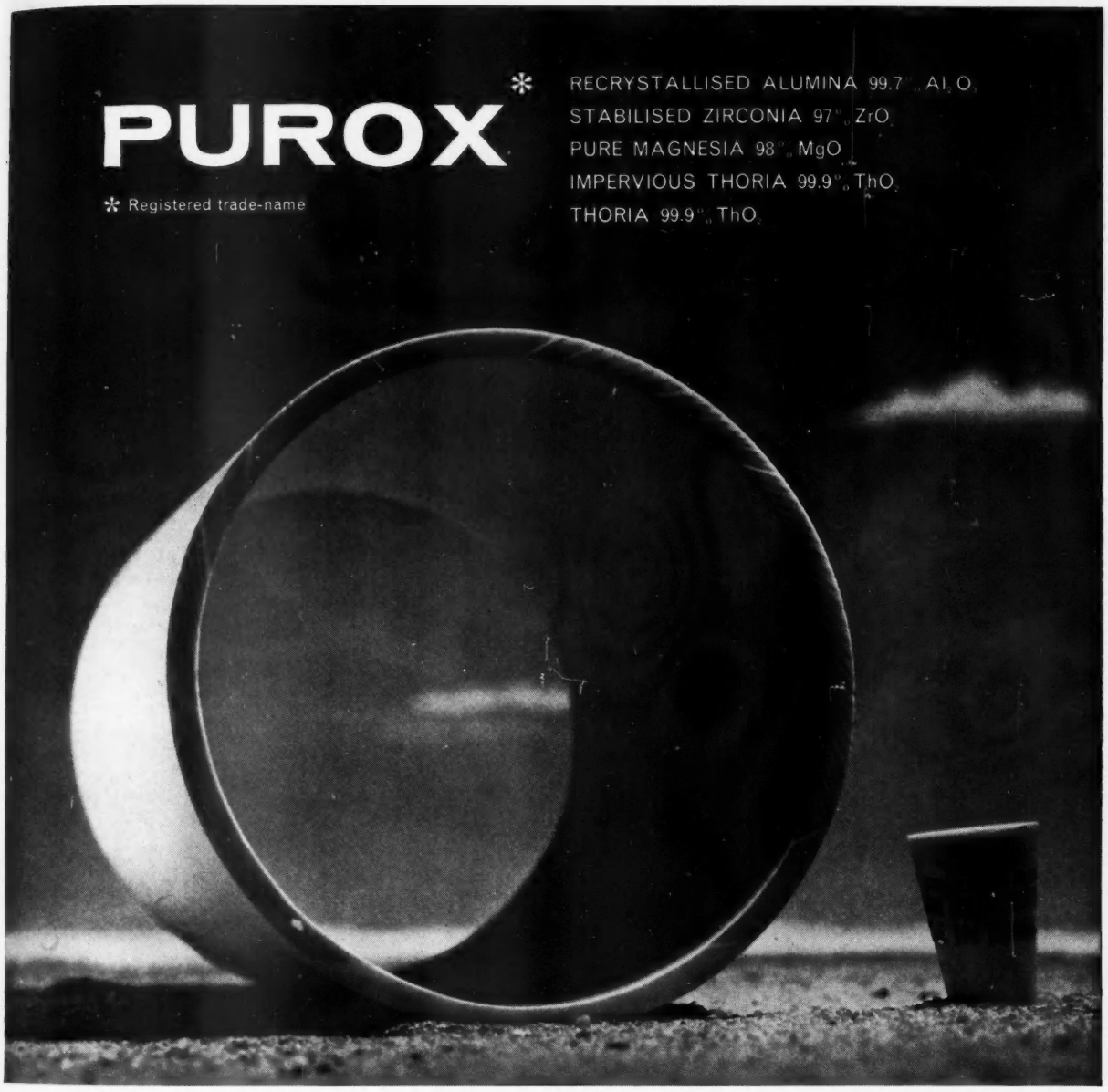

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